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**ESSAYS ON EXCHANGE RATE SHOCKS AND THE POLITICAL
ECONOMY OF LOCAL FISCAL POLICY IN BRAZIL**

A Dissertation Presented

by

RAPHAEL ROCHA GOUVÊA

Submitted to the Graduate School of the
University of Massachusetts Amherst in partial fulfillment
of the requirements for the degree of

DOCTOR OF PHILOSOPHY

February 2021

Department of Economics

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**ESSAYS ON EXCHANGE RATE SHOCKS AND THE POLITICAL
ECONOMY OF LOCAL FISCAL POLICY IN BRAZIL**

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by

RAPHAEL ROCHA GOUVÊA

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DEDICATION

To Mariana and Laís.

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I have always been lucky in my academic life. I say this because it is always possible to find good mentors from an academic point of view in any department of a good university. Much more uncommon, I realized over the years, it is finding mentors who will care deeply about your overall well being and not only about your academic and professional trajectory. For all their guidance and support along this journey, I am deeply indebted to my advisers and academic mentors.

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ABSTRACT

ESSAYS ON EXCHANGE RATE SHOCKS AND THE POLITICAL ECONOMY OF LOCAL FISCAL POLICY IN BRAZIL

FEBRUARY 2021

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Do exchange rate shocks have distributional consequences? Does employment respond to exchange rate shocks? Do political parties matter when it comes to governing cities? Each chapter of this dissertation attempts to answer one of these questions in the Brazilian context.

In the first chapter, titled **Large devaluations and inflation inequality: evidence from Brazil**, I show that prices of tradable goods/lower-priced varieties increase significantly more than the prices of nontradables/higher-priced varieties. These relative price changes may lead to inflation inequality when household consumption baskets are different across the distribution of income. Using Cravino and Levchenko [2017]’s methodology, we show that inflation of poor households in Brazil was at least 11 percentage points higher

than of the rich in the aftermath of the 2002 large devaluation. A detailed case study of the City of São Paulo estimates an inflation inequality ranging from 8 to 11 percentage points in the city.

In the second chapter, titled **Employment effects of real exchange rate shocks: evidence from Brazilian local labor markets**, I use local labor markets data over the period 1995-2016 for the Brazilian economy to study how employment responds to real exchange rate shocks. Exploiting regional variation in the intensity of real exchange rate shocks across local labor markets, I find that total employment increases in the short and medium run after a devaluation of the export-weighted real exchange rate. Meanwhile, a devaluation of the import-weighted real exchange rate decreases employment only in the short run. These effects are explained by the responses of the tradable sector as I find no significant effect of both measures of exchange rate shock on the nontradable sector. Within the tradable sector, manufacturing employment responds positively to export-weighted real exchange rate shocks both in the short and medium run, while I find no effect of these shocks on employment in the primary sector. Import-weighted real exchange rate shocks negatively affect both manufacturing and primary employment, but only in the short run.

In the third chapter, titled **Partisanship and local fiscal policy: evidence from Brazilian cities** and co-authored with Daniele Girardi, we study the role of partisanship in shaping local fiscal policy in Brazilian cities in the 2004-2016 period. Using a regression-discontinuity design, we find no effect of left-wing mayors on the size of the city government. We find a modest but robust positive effect of approximately 0.6 percentage points on the social expenditures share, which translates in a small (approximately 1 percent) increase in social expenditure per capita. The impact of left-wing mayors on social spending is stronger for lame-duck mayors and in cities receiving oil windfalls. These results suggest that Brazilian parties attempt to shape the allocation of municipal resources to favor their

respective electoral bases but their ability to do so is severely limited by factors such as institutional constraints and re-election concerns.

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CHAPTER 1

LARGE DEVALUATIONS AND INFLATION INEQUALITY: EVIDENCE FROM BRAZIL

1.1 Introduction

Figure 1.1 presents the evolution of the Brazilian trade-weighted nominal exchange rate (R\$/US\$) in recent years. From 1995 to 1998, the exchange rate was very stable as Brazil adopted an stabilization plan based on a pegged exchange rate regime. Since the collapse of the pegged regime, the exchange rate has been susceptible to high levels of volatility. In this paper, we discuss an often overlooked channel by which exchange rate shocks may lead to distributional consequences. We follow the methodology developed by Cravino and Levchenko [2017] to study the distributional consequences of large exchange rate shocks in Brazil.

Among the possible large devaluation¹ episodes we observe in Figure 1.1 (1999, 2002 and 2008), we focus on the 2002 episode for two reasons. First, we need a large devaluation episode that was sustained in the following years. Such devaluations usually produce changes in the relative price of tradable/nontradable goods, as initially documented by Burstein et al. [2003], which we will exploit in the empirical exercise. Only the large devaluations of 1999 and 2002 meet these criteria as the 2008 episode was not sustained. However, a major revision of the Brazilian consumer price index (CPI) in June 1999, only a few months after the devaluation, prevents us from studying this episode with the Cravino

¹We use the term devaluation and depreciation interchangeably throughout this paper.

and Levchenko [2017] methodology. Second, the 2002 devaluation episode was triggered by investors electoral concerns and, as such, the shock was exogenous to the economic fundamentals of the Brazilian economy at the moment. As described by Campello [2016], “Brazil’s long-term prospects seemed promising to analysts and investors alike” (p. 92) at the beginning of 2002, but “markets’ fears turned into outright panic as Lula’s leadership in the presidential race consolidated” (p. 95). The result of the Lula shock was a sharp fall in stock and bonds market, a halt on foreign capital inflows and, consequently, a large devaluation episode of the Brazilian exchange rate. In April 2002, the Brazilian trade-weighted exchange rate devalued 7 percent and the cumulative devaluation after 12 and 24 months were 44 and 40 percent, respectively. Moreover, consumer prices of tradable goods increased by 19 and 25 percent one and two years after the devaluation while prices of nontradable goods increased only by 10 and 16 percent over the same period.

The goal of the Cravino and Levchenko [2017]’s methodology is to calculate the changes in households cost of living following a large nominal exchange rate shock or, putting it another way, to measure the inflation inequality produced by the exchange rate shock. The methodology consists in a decomposition exercise of the consumer price index that highlights two types of effects. The *Across* effect explores differences in relative price changes and expenditure shares across products and across the income distribution. Data from the 2002-2003 consumer expenditure survey show that poorer households in Brazil consume relatively more tradable (especially food) than nontradable goods (such as services). Following a consumption pattern predicted by the Engel’s Law and present in the Brazilian data, households expenditure share of tradable goods decreases with the level of income, while the expenditure share of nontradables increases. As prices of tradable goods increased by a greater extent compared to prices of nontradable goods after the large devaluation, we expect that households at the bottom of the income distribution faced higher

increases in their cost of living than households at the top. The *Within* effect explores differences in price changes and expenditure shares within product categories. Lower quality goods purchased from lower-end retail stores are consumed in a higher proportion by low-income households than high-income households. Then, if prices of lower quality goods increase relatively more than high-quality goods within product categories, the price level of low-income households will increase relatively more than high-income households.

We estimate that the difference in inflation due to the *Across* effect of households situated in the first and tenth deciles of the income distribution was 11 percentage points two years after the shock. This translates into an increase of the cost of living that was 1.52 times higher for households in the bottom of the income distribution compared to the top.

The computation of the within effect requires observing the price quotes of each variety used to construct the consumer price index. These data are not available to the public for the official Brazilian consumer price index. To circumvent this problem, we compute the *Within* effect using data from a consumer price index for the City of São Paulo, which is among the most traditional and broadly used indices in Brazil. We, then, proceed as follows. First, we show that the pattern of relative price changes in the aftermath of the devaluation is the same using IPCA (the official consumer price index) or IPC-FIPE (the CPI for the City of São Paulo). Second, we compute the *Across* price index using the IPC-FIPE and find that the results are qualitatively the same as the ones obtained using IPCA: households in the first decile of income faced higher inflation compared to households in the tenth decile (of around 3 percentage points) after the large devaluation shock. After documenting that both IPCA and IPC-FIPE deliver similar results for the *Across* effect, we use the city of São Paulo as a study case for the *Within* effect.

We estimate that the difference in inflation due to the *Within* effect of households in the first and tenth deciles of the income distribution in the City of São Paulo was between 2 and 5 percentage points. The increase in the cost of living of poor households relative to the rich was 1.11 times higher in the most conservative case and 1.40 times higher in the less conservative one. For the City of São Paulo, we can also estimate the combined effect which ranges from 8 to 11 percentage points and translates into an increase in the cost of living of the poor that is 1.39 to 1.67 times higher than the increase of the rich.

This paper is related to the vast literature on the relationship between prices and exchange rates, especially with the literature on exchange rate pass-through. As reviewed in greater detail by Burstein and Gopinath [2015], two stylized facts have been produced in the exchange rate pass-through literature. First, pass-through into consumer prices is lower than into border prices [Campa and Goldberg, 2005, Burstein and Gopinath, 2015]. Second, border prices respond partially to exchange rate shocks irrespective of the currency they are set [Gopinath et al., 2010, Gopinath and Itskhoki, 2010, Gopinath, 2015].²

The empirical approach of this paper is even closer to the literature that exploits large nominal exchange rate changes as a source of identification. The general idea behind this identification strategy is that large exchange rate shocks — or, at least, their timing — are usually exogenous to the local economy. This strategy has been used to study exchange rate pass-through and changes in relative prices [Burstein et al., 2003, 2005, 2007], prices and

²It is important at this point to clarify some terminology used to refer to different price measures. Consumer or retail prices, measured by Consumer Price Indices (CPI), are prices paid by consumers when buying goods and services. Consumer prices are, then, prices charged by the retail sector. The Producer Price Indices (PPI) measure prices of production and, besides consumption goods, they also include intermediate and investment goods. Border prices or prices “at the dock” are prices of actually traded goods. They can be measured using Import Price Indices (IPI) or Export Price Indices (EPI).

consumer behavior [Auer et al., 2017, Burstein and Neumeyer, 2010], and distributional issues [Cravino and Levchenko, 2017].³

Finally, it is important to highlight that the channel we study in this paper is not the only one going from exchange rate shocks to households welfare. Besides the distributional consequences of nominal exchange rate shocks that happen through the price of consumption goods, devaluations may also affect nominal wages and employment levels, even though the sign of the effects of a real devaluation on these variables is usually ambiguous from a theoretical point of view [Alejandro, 1963, Krugman and Taylor, 1978, Agénor and Montiel, 2015, Gandolfo, 2016]. Our results should, then, be understood as derived from a partial equilibrium model where nominal wages and employment levels are taken as given. This assumption justifies the short-run nature of the empirical exercises presented in this paper which are restricted to two years after the initial shock. Appendix A presents a simple pricing framework that helps to clarify the main assumptions behind the empirical exercise.

The rest of the paper is organized as follows. Section 1.2 discusses the theoretical framework and price indices definitions. Data and an empirical overview are presented in Section 3.3. Section 1.4 brings the main empirical results of the paper, which ends with some concluding remarks.

1.2 Conceptual Framework and Price Indices Definitions

In this section, we present the conceptual framework and price indices definition following closely the discussion in Cravino and Levchenko [2017].

³Large exchange rate shocks have also been used to study other topics in international economics, such as trade, quality upgrading and wage inequality [Verhoogen, 2008, Araújo and Paz, 2014], quality and exchange rate pass-through [Goetz and Rodnyansky, 2016], employment, domestic revenue and profitability of exporting firms [Rodnyansky, 2017] to mention a few papers.

1.2.1 Conceptual framework

Assume that the indirect utility of household h , its income and the vector of prices are given by V_t^h , W_t^h and $P_{g,t}$. In this case, the proportional change in welfare given a change in income and the vector of prices can be approximated by

$$\hat{V}_t^h = \hat{W}_t^h - \sum_{g \in G} \omega_g^h \hat{P}_{g,t} \quad (1.1)$$

where a hat over a variable indicates its cumulative growth rate, g indexes goods and ω_g^h are household-specific expenditure shares. As shown by Cravino and Levchenko [2017], if we sum and subtract $\omega_g \hat{P}_{g,t}$ to the right-hand side, where ω_g is the economy-wide expenditure share on good g , equation (1.1) can be written as

$$\hat{V}_t^h = \underbrace{\hat{W}_t^h - \sum_{g \in G} \omega_g \hat{P}_{g,t}}_{\text{homothetic-utility } \hat{V}} - \underbrace{\sum_{g \in G} (\omega_g^h - \omega_g) \hat{P}_{g,t}}_{\text{Cov}(\hat{P}_{g,t}, \omega_g^h - \omega)} \quad (1.2)$$

Equation (1.2) makes explicit the source of the distributional effects of the price changes. The first term captures the change in welfare if households expenditure shares in every good g were the same and their utility homothetic. The distributional effect is captured by the second term which is a covariance between price changes and the relative expenditure shares. Then, if household h relative expenditure shares are higher in goods whose prices increase by a greater extent, household h has a greater decrease in welfare than the average household. As pointed out by Cravino and Levchenko [2017], these equations also show that the results can be interpreted either as heterogeneity in costs of living or in the compensating variation across households.⁴ To measure the extent of these heterogeneous

⁴The compensating variation is equal to the change in income required to keep welfare unchanged given a vector of price changes.

changes in the cost of living across households, the authors propose a decomposition of the overall price index in two main sub-indices and a covariance term as follows.

1.2.2 Price indices definitions

Define the change in the aggregate price index as:

$$\hat{P}_t \equiv \sum_{g \in G} \omega_g \hat{P}_{g,t} \quad (1.3)$$

where $g \in G$ is a good category and, as before, ω_g is the economy-wide expenditure share on good g . Assume that within each good category, there are v_g varieties. Then, the change in the price index for good category g with V_g varieties is given by:

$$\hat{P}_{g,t} \equiv \frac{1}{V_g} \sum_{v_g \in g} \hat{P}_{v_g,t} \quad (1.4)$$

If we assume that households have different expenditure shares across and within product categories, we can define the household-specific price index as:

$$\hat{P}_t^h \equiv \sum_{g \in G} \omega_g^h \hat{P}_{g,t}^h \quad (1.5)$$

where ω_g^h is the expenditure share of household h in category g and $\hat{P}_{g,t}^h$ is the change in the price sub-index of good g . As households consume different varieties, this price sub-index varies by household as in:

$$\hat{P}_{g,t}^h \equiv \sum_{v_g} s_{v_g}^h \hat{P}_{v_g,t} \quad (1.6)$$

where s_{v_g} is the household expenditure share in variety v_g within product category g and $\hat{P}_{v_g,t}$ is the economy-wide change in the price of variety v_g of good g . $\hat{P}_{g,t}^h$ will then vary across households if they consume different varieties within categories.

The *Across* and *Within* price indices can be defined as follows:

$$\hat{P}_{Across,t} \equiv \sum_{g \in G} \omega_g^h \hat{P}_{g,t} \quad (1.7)$$

$$\hat{P}_{Within,t} \equiv \sum_{g \in G} \omega_g \hat{P}_{g,t}^h \quad (1.8)$$

Therefore, while equation (1.7) assumes household-specific expenditure shares and economy-wide price indices for goods g , equation (1.8) assumes economy-wide expenditure shares and household-specific price indices for varieties v_g .

Using the previous definitions, it is possible to write the change in the price index of household h as:

$$\hat{P}_t^h = \underbrace{\sum_{g \in G} \omega_g^h \hat{P}_{g,t}}_{\hat{P}_{Across,t}} + \underbrace{\sum_{g \in G} \omega_g \hat{P}_{g,t}^h}_{\hat{P}_{Within,t}} + \underbrace{\sum_{g \in G} (\omega_g^h - \omega_g) (\hat{P}_{g,t}^h - \hat{P}_{g,t})}_{\hat{P}_{Cov,t}} - \underbrace{\sum_{g \in G} \omega_g \hat{P}_{g,t}}_{\hat{P}_t} \quad (1.9)$$

and the difference in price indices of two households at different points of the income distribution as:

$$\Delta \hat{P}_t = \Delta \hat{P}_{Across,t} + \Delta \hat{P}_{Within,t} + \Delta \hat{P}_{Cov,t} \quad (1.10)$$

where Δ denotes a cross-sectional difference between household h and h' .

In the next sections, we provide estimates for the *Across* and *Within* price indices. However, for some product categories, prices of identical goods cannot be observed continuously over time and an additional hypothesis is required to obtain a *Within* price index representative of the whole economy. In the empirical implementation, Cravino and Levchenko [2017] suggest using a conservative and liberal version of the *Within* price index. In the conservative version is assumed that the relative price of varieties remained constant for

the missing generic categories, while the liberal version assumes that the change in relative prices of these missing categories is equal to the weighted average price change of the observed varieties. The conservative and liberal versions are, then, defined by:

$$\hat{P}_{Within-C,t} \equiv \sum_{g \in G_M} \omega_g \hat{P}_{g,t}^h + \sum_{g \in G_U} \omega_g \hat{P}_{g,t} \quad (1.11)$$

and

$$\hat{P}_{Within-L,t} \equiv \sum_{g \in G_M} \omega_g \hat{P}_{g,t}^h + \sum_{g \in G_U} \omega_g \hat{P}_{g,t} \frac{\sum_{g \in G_M} \omega_g \hat{P}_{g,t}^h}{\sum_{g \in G_M} \omega_g \hat{P}_{g,t}} \quad (1.12)$$

where G_M is the set of categories for which identical varieties are measured continuously through time and G_U is the set of categories for which identical varieties are not continuously observed.

1.3 Data and Empirical Overview

Implementing Cravino and Levchenko [2017]’s decomposition requires two types of data: consumer prices and household expenditures. In addition to consumer price indices, the *Within* effect also requires observing the surveyed price quotes of different varieties. Given that these are not public available for IPCA, we use data from IPC-FIPE to quantify the *Within* effect in the City of São Paulo.⁵

1.3.1 IBGE data

IBGE, the Brazilian national bureau of statistics, produces consumer price indices since 1979 and IPCA is the official measure of overall inflation in Brazil. The weighting

⁵There are many different CPIs in Brazil, produced by different institutions and having different targeted populations and regional coverage. In addition to IPCA and IPC-FIPE, IPC-BR from Fundação Getúlio Vargas (FVG) is also among the most traditional CPIs in Brazil. For a summary of the most important methodological differences among IPCA, IPC-FIPE and IPC-Br, see BCB [2004].

structure of the IPCA basket is constructed and updated by IBGE using micro data from consumer expenditure surveys. We use data on total income and total expenditure from the *Pesquisa de Orçamentos Familiares 2002-2003* (henceforth POF 2002-2003) to obtain the consumption pattern of households across product categories.⁶ Using information from the POFs, IBGE sets the criteria for a product to be included in the IPCA basket and computes the weighting structure of the overall price index. IBGE, then, surveys the prices of these products and publishes the results following a hierarchical classification that aggregates each product into four categories: group (1 digit), subgroup (2 digits), item (4 digits) and subitem (7 digits). For example, **orange** is a subitem of the item **fruits**, which belongs to the subgroup **food at home** that is part of the group **food and beverages**. The IPCA subindices are then available for 8 groups, 19 subgroups, 52 items and 512 subitems for the period under study.⁷

1.3.2 FIPE Data

The consumer price index of the City of São Paulo started in January 1939 and its time series is the longest for a consumer price index in Brazil. Its calculation was carried out by the City of São Paulo until 1968, when it was transferred to FIPE, a nonprofit organization created to support the Economics Department of the University of São Paulo.⁸

⁶Since IBGE started producing CPIs, there were five of such surveys in Brazil: the *Estudo Nacional de Despesa Familiar* (ENDEF) of 1974-1975 and the POFs of 1987-1988, 1995-1996, 2002-2003 and 2008-2009. Besides ENDEF, POF 2002-2003 was the first nationally representative consumer expenditure survey in Brazil. For a historical overview and the main differences among these consumer expenditure surveys, see Diniz et al. [2007].

⁷IBGE updates the weighting structure a few years after each POF, *e.g.*, the IPCA revision after POF 2002-2003 occurred in 2006. Since the stabilization of the economy with *Plano Real*, there were three revisions. Martinez [2014] discusses in detail their differences and provide a compatibilization table from the changes after 1999.

⁸Rizzieri and Carmo [2006] present the history and methodology of the IPC-FIPE from its beginning until the 1994 revision. For the most recent revisions, see Carmo [1999], de Lima et al. [2011] and Chagas et al. [2015].

IPC-FIPE measures the cost of living in the City of São Paulo and its consumption basket is constructed using specific consumer expenditure surveys carried out by FIPE. I use data on total income and total expenditure from the *Pesquisa de Orçamentos Familiares 1998-1999* (henceforth POF 1998-1999), the closest to the period under study, to obtain the consumption pattern across product categories of households living in the City of São Paulo. After determining the weighting structure of the index, FIPE surveys the price levels of the products included in the CPI basket and I had access to this proprietary data set at the product-outlet level with information over the period 1998 and 2007. In the FIPE data, I can then observe monthly average price quotes with a unique product-outlet identifier. As in Cravino and Levchenko [2017], we consider each product-outlet information a specific variety. Varieties are grouped by FIPE using a different hierarchical classification from IBGE but this classification also has four levels. In the period under study, there are 7 groups, 29 subgroups, 54 items and 463 subitems.

1.3.3 Empirical Overview

The empirical exercises performed in the next section are based on two very simple ideas. The first is that relative price changes in the aftermath of the 2002 Brazilian depreciation followed the main stylized facts of the exchange rate pass-through literature, *i.e.*, that the increase in the prices of tradable goods is higher than the prices of nontradable goods after a devaluation[Burstein and Gopinath, 2015]. The second is that household consumption pattern in Brazil follows the pattern predicted by the Engel’s Law.

Figure 1.2 presents price indices normalized to 1 in April 2002, the month before the depreciation. It shows that prices at the dock tracked closely the exchange rate movement in the period, while prices of tradable goods increased by a greater extent than prices of nontradables. In fact, the exchange rate shock created a gap between the price levels of tradable and nontradable goods that started three months after the depreciation and was

sustained afterward. It is also important to highlight that this result does not depend on the choice of the consumer price index. Both IPCA and IPC-FIPE showed the same pattern of relative price changes and are also numeric similar, giving us more confidence that the results for the City of São Paulo may be representative of what happened for the country as a whole when computing the *Within* effect.

Figure 1.2 plots expenditure shares in tradable and nontradable goods by income decile for Brazil⁹. The consumption pattern observed in the figure is clear: the higher the household level of income, the smaller its expenditure share in tradables and the higher its expenditure share in nontradables as expected by the Engel's Law¹⁰.

In summary, the evidence presented in this section suggest that the relative price movements after the 2002 depreciation and the households pattern of consumption may have produced important heterogeneity in the cost of living of Brazilian households. The next section corroborates this suggestion and presents empirical estimates for the distributional effects of this devaluation episode by computing the *Across* effect at the level of the country and the *Across*, *Within* and *Combined* effect for the City of São Paulo.

1.4 Empirical Results

1.4.1 The Across Effect

Table 1.1 reports the *Across* price indices, computed as in equation (1.7), one and two years after the 2002 depreciation for each decile of income. In panel A, the price index is computed at 1 digit, *i.e* nine groups of the IPCA and seven groups of the IPC-FIPE. Panel

⁹To classify goods in the consumer expenditure survey as tradable and nontradable, we used the Brazilian Central Bank classification which was made available by Martinez [2014]. Unfortunately, we do not have a similar classification for the consumer expenditure survey of the City of São Paulo.

¹⁰Hoffmann [2007] presents a more formal statistical analysis of the validity of the Engel's Law in Brazil using data from POF 2002-2003.

B reports results computed at 7 digits, *i.e.* 512 subitems of the IPCA and 463 subitems of the IPC-FIPE. In both cases, the results show that there is important heterogeneity in the changes in price level across the distribution of income.

The *Across* price index for Brazil at the 1 digit level changed by 25 percent for households at the first decile compared to the 22 percent for households in the 10th decile. This difference is more striking at 7 digits, when households at the top decile observed an increase in the across price index of 21 percent, while the change was 32 percent for households at the bottom.

The results are qualitatively the same for the City of São Paulo. Even though numerically they are slightly smaller, this was expected as overall inflation was smaller in the City of São Paulo as shown in the column where the actual figures of the IPCA and IPC-FIPE are reported. In the City of São Paulo, the difference in price changes between the first and tenth deciles are 3 percentage points.

Figure 1.3 presents the evolution of these price indices during the two-year window after and six months before the devaluation. It shows that the gaps in price changes among deciles started three months after the initial shock. More important, the figure shows no differential pre-trends among deciles suggesting that the inflation inequality was indeed driven by the large devaluation.

Figure 1.4 presents the results when the index is computed at the household level using the lower level of aggregation of the consumer price indices. Similarly to the decile results, the figure shows a negative relationship between household income and the *Across* price index.

Appendix A presents two robustness exercise for the *Across* price index. First, we calculate the *Across* price index by each of the nine metropolitan regions for which IBGE calculates specific consumer price indices. As we can see in table A.3, in all regions the poor

households experience a much larger increase in inflation after a large devaluation of the exchange rate. Second, we calculate the *Across* price index using end-of-periods weights to assess if the results change due to differential ability to substitute consumption across categories between poor and rich households. Unfortunately, the next available consumer expenditure survey was only in 2008-2009, a long time after the devaluation. Therefore, results reported in table A.4 should be taken with some caution, but they also show that households in the lower end of the income distribution experienced a much higher rate of inflation following the devaluation than the households in the top.

1.4.2 The Within Effect

We cannot calculate the *Within* price index for the Brazilian economy because the price quotes used to construct the IPCA are not available. In this section, we use the City of São Paulo as a study case for the *Within* effect. Even though we observe price quotes of each variety in the IPC-FIPE, we do not have information on household spending by varieties. For this reason, we calculate price indices for high and low-priced varieties and assume, following the evidence presented in Cravino and Levchenko [2017] and in Appendix A, that high-priced varieties are consumed by rich households (in 10th decile of the income distribution) while low-priced ones are consumed by poor households (in the 1st decile). Two criteria are used to classify varieties as high or low-priced: first, they are classified as high(low)-priced varieties when their average price in the 12 months before the devaluation is above(below) the median average price of the category; second, they are classified as high(low)-priced varieties when their average price in the 12 months before the devaluation is in the fourth (first) quartile of the distribution of average prices of the category.

As mentioned in section 1.2, we cannot observe the price quotes of some individual goods continuously over time. This is the case for 201 product categories. For the other

325, we can observe at least two varieties continuously over time and these categories represent 66 percent of the IPC-FIPE. Due to the missing categories, we compute the *Within* price index representative of the whole City of São Paulo using the conservative and liberal versions of the *Within* price index as described in equations (1.11) and (1.12), respectively. To recap: in the conservative version we assume that the relative price of the cheap versus expensive varieties remained constant, while in the liberal version we assume that the relative price of the cheap versus expensive varieties was equal to the weighted average price change of the observed categories.

Figure 1.5 plots the evolution of the *Within* price indices using the two criteria to define high and low-priced varieties. As in the case of the *Across* price index, the figure shows no pre-trends before the devaluation and the price indices start to diverge after the shock. Table 1.2 reports the results of the price indices one and two years after the devaluation. As we can see, using the median criteria to sort varieties into high and low priced, the price index of low-priced varieties is about 2 to 5 percentage points higher than the price index of high-priced varieties depending on whether we use the conservative or liberal version. Using the quartile definition, we estimate a price difference ranging from 3 to 8 percentage points.

As a robustness exercise, Appendix A reports results for the *Within* effect calculated using a more restrictive definition of individual products to be considered for inclusion in the set of observed categories. In this case, only products whose prices are quoted in a specific measurement unit (like kg or grams) are included in this set. Even though this is a more restrictive criteria, leading to a coverage of 35 percent of the overall CPI, it has the advantage of excluding categories for which prices are quoted using a “sample” of the product that is available when prices are collected. As we can see in the appendix, the

results do not change qualitatively, but the estimated price differences between high and low-priced varieties are slightly higher.

1.4.3 The Combined Effect

This section presents results for the price index that combines the *Across*, the *Within* and the covariance effects. Due to the categories with missing varieties, we use the same hypothesis to calculate the conservative and liberal version of the price index. The combined price index is given by equation (1.3) where the spending weights and price index vary by household. We report the results for representative low-income and high-income households where the former has across-good expenditure shares of a household in the first income decile, while the latter has across-good expenditure shares of a household in the tenth decile. As in the case of the *Within* price index, we assume that households at the bottom consume low-priced varieties and that households at the top consume high-priced varieties.

Figure 1.6 presents the evolution of the *Combined* price index using the median and quartile criteria to define low and high-priced varieties. The figure shows that the price index of poor households diverge from the the price index of rich households after the devaluation. Again, we cannot identify any pre-trends in the year before the devaluation as the price index of poor and rich households were very close to each other.

Table 1.3 reports the difference in inflation one and two years after the devaluation. For households at the bottom of the income distribution, we estimate that inflation two years after the devaluation ranged from 28 to 32 percent. For households at the top, inflation in the same period ranged from nearly 20 to 19 percent. Using the more conservative assumptions — conservative version of the price index and varieties split into categories according to the median — inflation two years after the devaluation was 8 percentage points higher for poor households. Using the more liberal assumptions — liberal version

of the price index and varieties split into categories according to first and fourth quartiles — the same difference in inflation was 13 percentage points.

Similarly to the case of the *Within* price index, Appendix A presents results of using a more restrictive definition of individual products. Again, the results reported in Table A.6 and Figure A.3 do not change qualitatively and the price differences two years after the devaluation is higher for households at the bottom compared to households at the top.

1.5 Benchmark, mechanism and confounding factors

To place our results in context, it is interesting to compare them with the findings in Cravino and Levchenko [2017]. Similarly to their study for Mexico, we find that households in the lower end of the income distribution faced higher rates of inflation in the aftermath of the depreciation. Moreover, the more disaggregated the price information used to compute the price indices, the larger the price change differences between households at different points of the income distribution.

Quantitatively, even though the size of the initial devaluation and the overall pass-through was larger in Mexico, the distributional consequences of the devaluation were stronger in Brazil. Cravino and Levchenko [2017, pp. 11] estimate that the change in the across price index was 1.25 times higher for households at the bottom of the income distribution than at the top in the 1994 Mexican devaluation episode. Restricting the analysis to Mexico City, they find that this price change was 1.17 times higher for the first decile. We estimate this difference to be 1.52 and 1.18 for Brazil and City of São Paulo, respectively. For the within price index, Cravino and Levchenko [2017] estimate that the price change was 1.1 to 1.28 times higher for households in the bottom of the income distribution using, respectively, the most conservative and the most liberal assumptions. For the combined effect, these figures were 1.28 and 1.45. For the City of São Paulo, we

find that these price differences were 1.11 to 1.4 (conservative to liberal) for the within price index and 1.39 to 1.67 (conservative to liberal) for the combined price index .

Compared to Mexico, two things seem to drive the fact that the distributional impacts are larger in Brazil even with a smaller overall exchange rate pass-through. First, both consumer price indices used in my study are more disaggregated (have more items) than the Mexican CPI. Second, and maybe even more important, income is significantly more concentrated in Brazil leading to larger differences in consumption expenditure shares between poor and rich households. While in the 1994 large devaluation episode in Mexico the 90-10 ratio was 23 and 20 for Mexico and the Mexico City, Table A.1 shows that these figures were 41 for Brazil and 31 for the City of São Paulo in the 2002 Brazilian episode.

Throughout this paper, we have interpreted the inflation inequality in the two years after the 2002 devaluation as a consequence of the devaluation itself. The reason for that, besides the exogeneity of the devaluation discussed in the introduction, is that the mechanism leading to these effects are well understood in the literature of international prices. As shown theoretically and with time series data by Burstein et al. [2005] and with disaggregated data by Cravino and Levchenko [2017], the fact that there is no complete pass-through after the devaluation and that we observe heterogeneous price changes across goods can be explained by heterogeneity in the weight of distribution of costs — *i.e* costs of retail services, marketing, advertising and distribution services — and local goods in retail prices (see Appendix A for details).

However, even though there is evidence on this specific mechanism, one possible objection to our results is that they might be driven by other confounding factors and not the 2002 large devaluation of the Brazilian Real. Although we cannot provide more rigorous tests on this issue, some observations and the timeline of events suggest that other likely explanations do not seem to drive the results. First, our study case of the City

of São Paulo and the regional results of IPCA in Appendix A suggest that the inflation inequality we observe does not stem from any type of regional shock, *e.g.* state/municipal fiscal policy or local decisions about regulated prices like public transportation. Second, the timeline of the major events and the effects we find do not suggest that other possible confounders at the national level, which may have affected differentially the demand for tradables/nontradables and low-priced/high-priced varieties, are driving the results either. For example, the heterogeneous effects on inflation already show up in 2002, when the transition in the federal government had not happened yet. In its turn, the first year of Lula’s presidency was very conservative on the macroeconomic front, with high interest rates and with the government delivering a fiscal primary surplus higher than the target. Even though the minimum wage has increased 20% in nominal terms, the increase happened in April 1st of 2003. At that point, only due to the *Across* effect, the inflation faced by the first decile was already 10 percentage points higher for the average household in the first decile compared to the average household in the tenth decile as shown in Table 1.1. Moreover, social expenditures targeting the poor (Bolsa Familia) started growing especially fast only after 2003 when sharp increases in commodity prices created more fiscal space [Campello, 2016].

1.6 Concluding Remarks

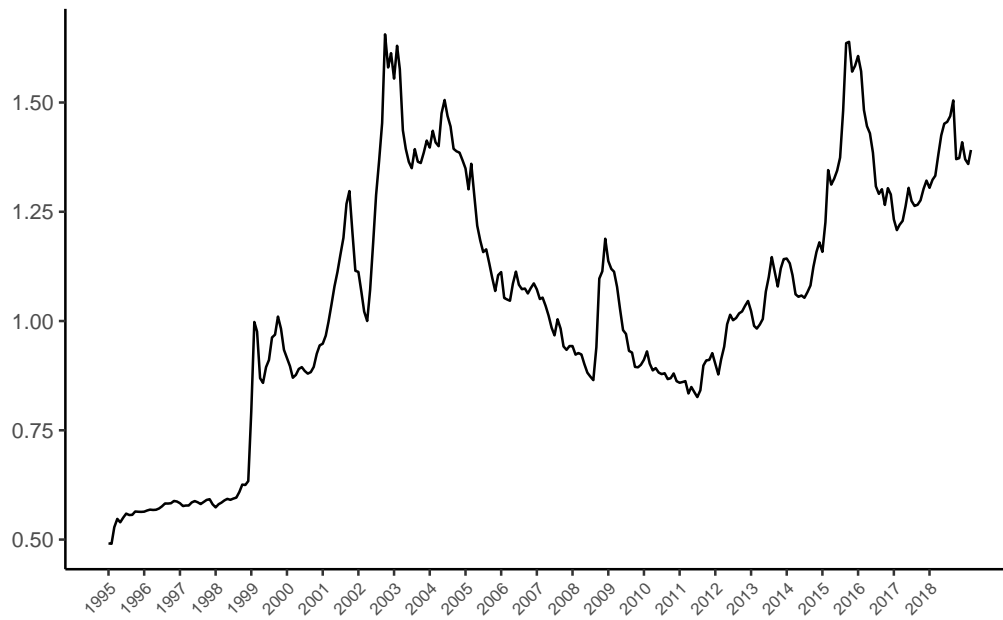
This paper has studied the distributional consequences of the 2002 Brazilian large exchange rate shock. The difference in the changes of households cost of living documented in this paper is driven by the relative price changes brought about by the devaluation and differences in consumption pattern among households across different points of the income distribution. Following the methodology first adopted in Cravino and Levchenko [2017], we show that the inflation rate for the average Brazilian households at the bottom decile

of the income distribution was 11 percentage points higher than for the average household at the top decile two years after the 2002 devaluation due to the *Across* effect. For the City of São Paulo, this difference was equal to 3 percentage points. My study case of the City of São Paulo also points to important distributional impacts along the *Within* effect dimension with differences in inflation in the range of 2 and 5 percentage points. The *Combined* effect for the City of São Paulo ranges from 8 to 11 percentage points.

Even though the analysis is silent in terms of the evolution of nominal income, it allows us to draw some inference in terms of inequality of real incomes. Given our most conservative estimate for the City of São Paulo, the results imply that nominal income in the first decile must have increased at least 1.39 times more than the income of the tenth decile just to keep the relative position of the first decile if we consider real income measures. Using only the *Across* effect, we find that this increase would have to be at least 1.52 times higher for an average Brazilian household in the bottom of the income distribution. How the devaluation affected nominal income via the employment and compensation channels requires future research.

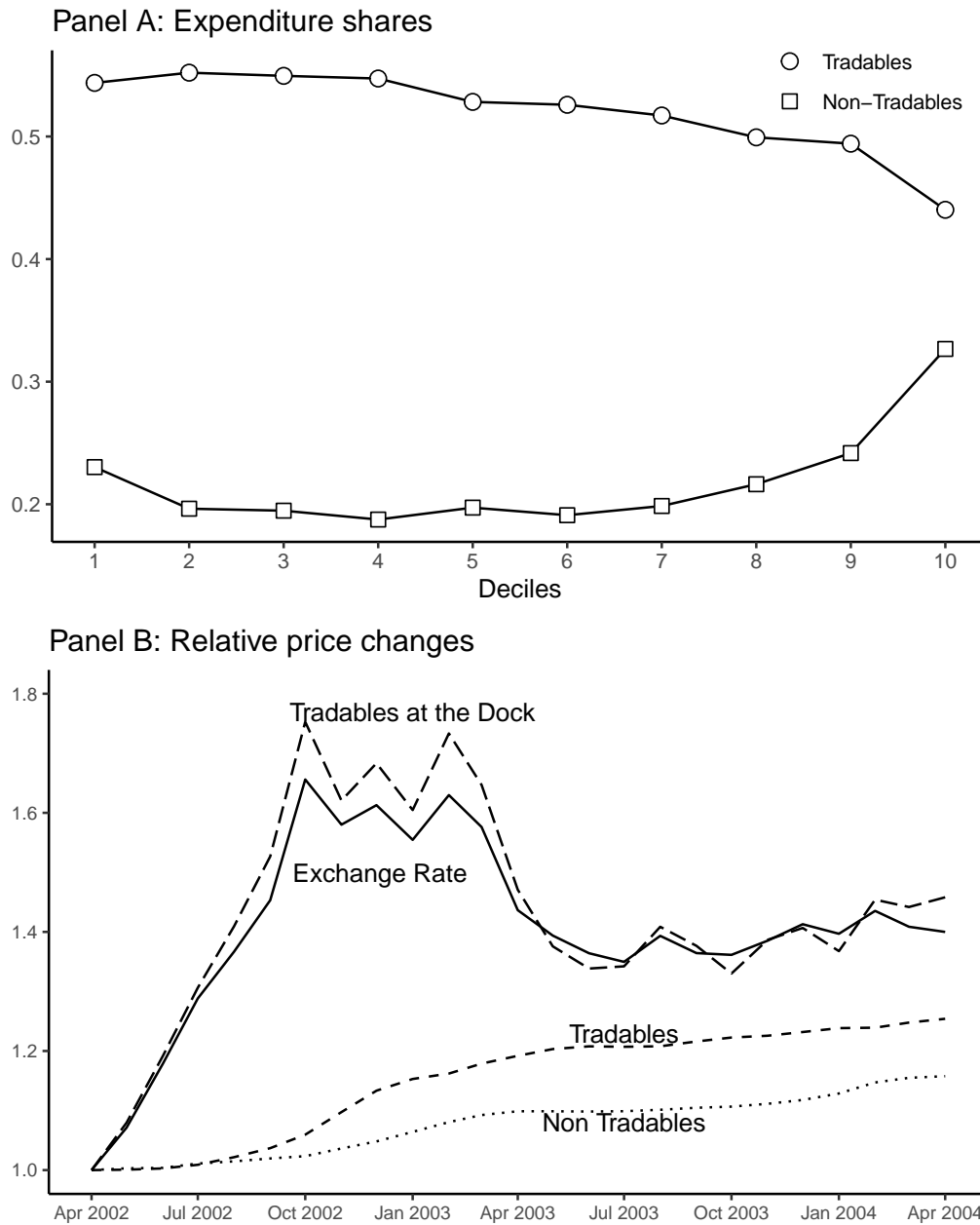
FIGURES AND TABLES

Figure 1.1: Trade-weighted Nominal Exchange Rate (Apr 2002=1)



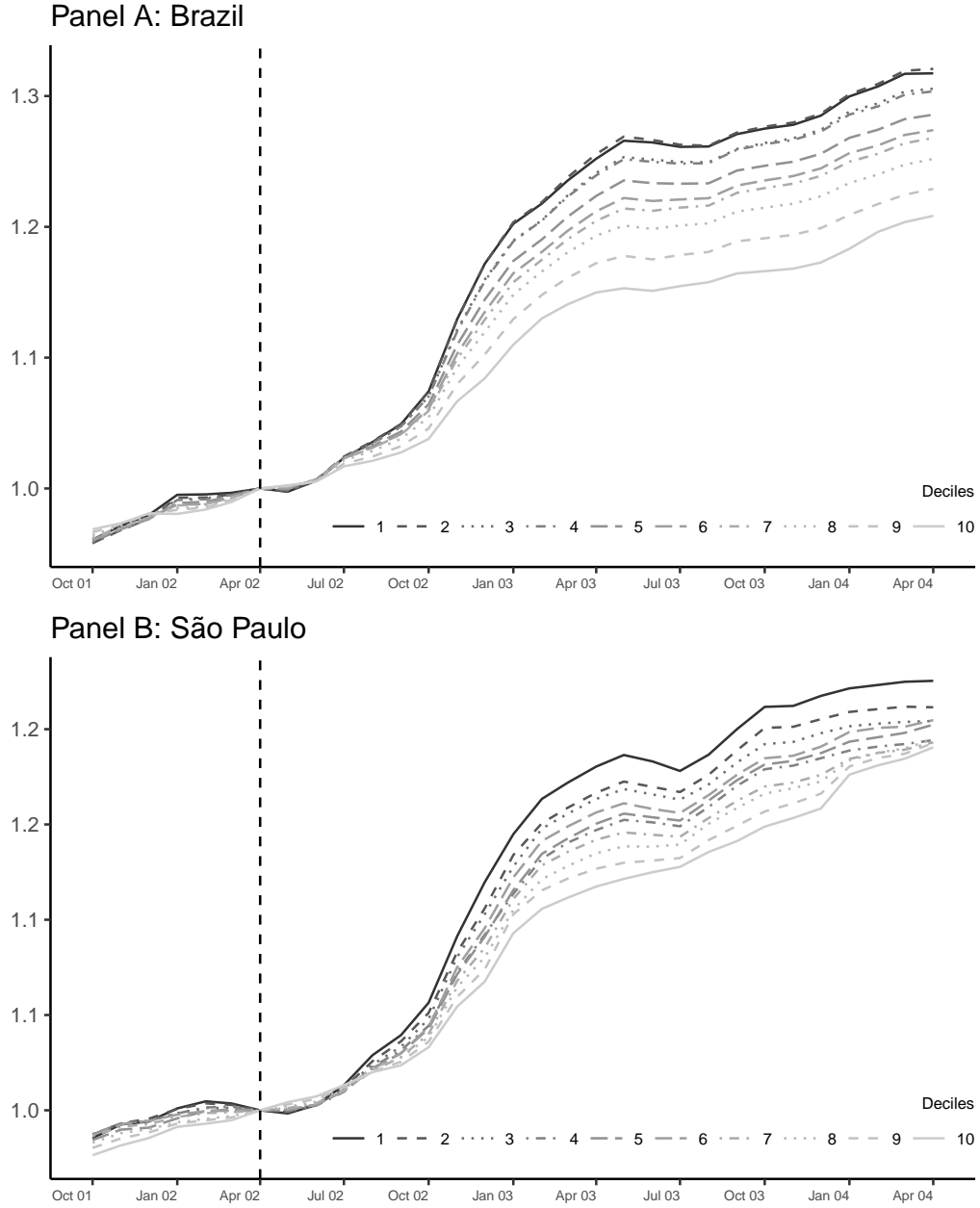
Source: BIS

Figure 1.2: Expenditure shares by income decile



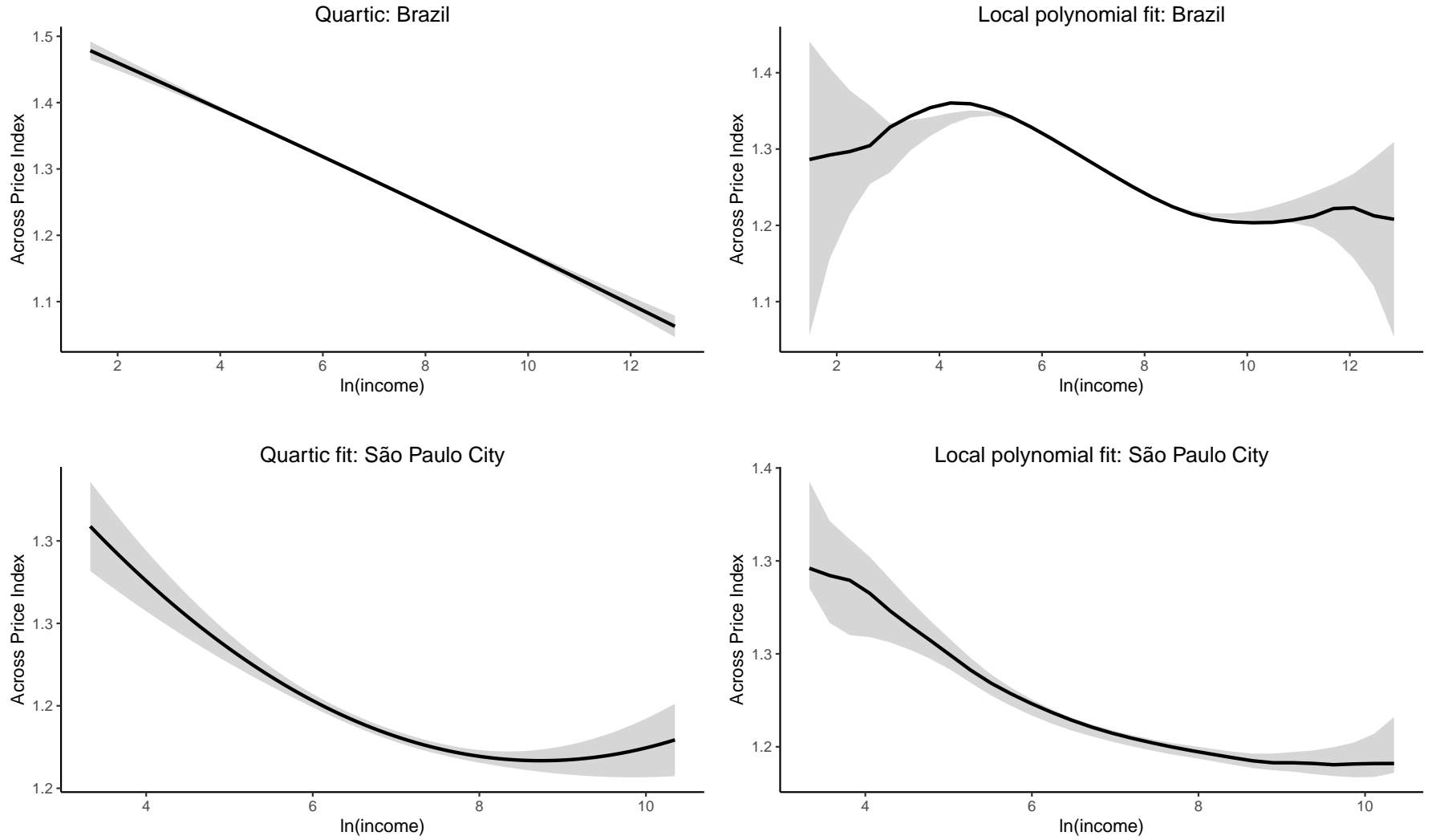
Notes: Expenditure shares of tradables and non-tradables are from POF 2002. Trade-weighted nominal exchange rate is from the BIS, price of tradables at the dock is the Import Price Index from FUNCEX, price of tradables and nontradables are from IBGE. All indices are normalized to 1 in April 2002, the month before the devaluation. Source: Author's calculation.

Figure 1.3: The evolution of the across price indices by income decile



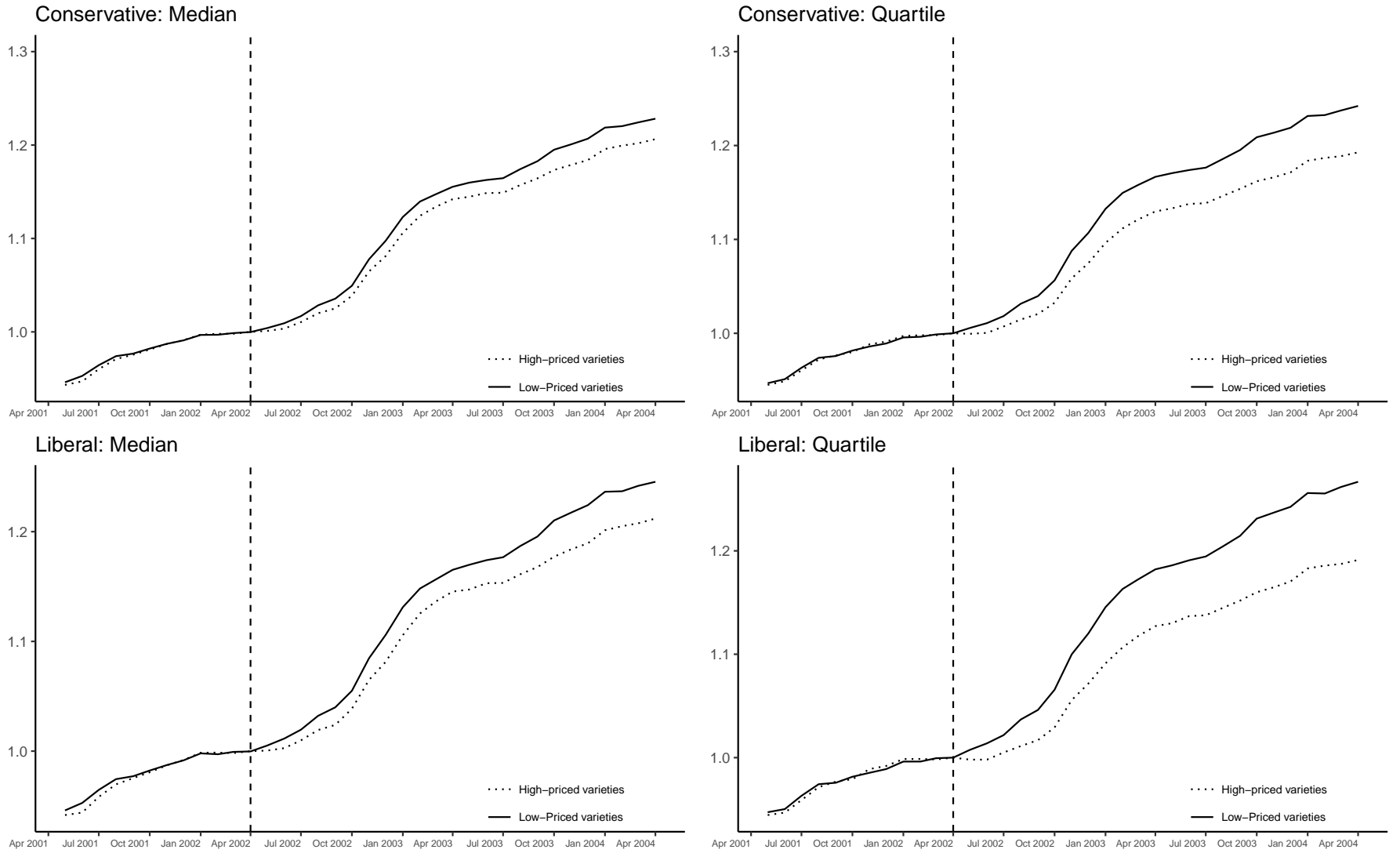
Notes: The across price indices are computed as a weighted average of economy-wide price indexes for each 512 product categories (P_g) and household expenditure shares by income decile (ω_g^h): $\hat{P}_{Across,t} \equiv \sum_{g \in G} \omega_g^h \hat{P}_{g,t}$. The vertical line marks the start of the devaluation episode.

Figure 1.4: The across price index at the household level



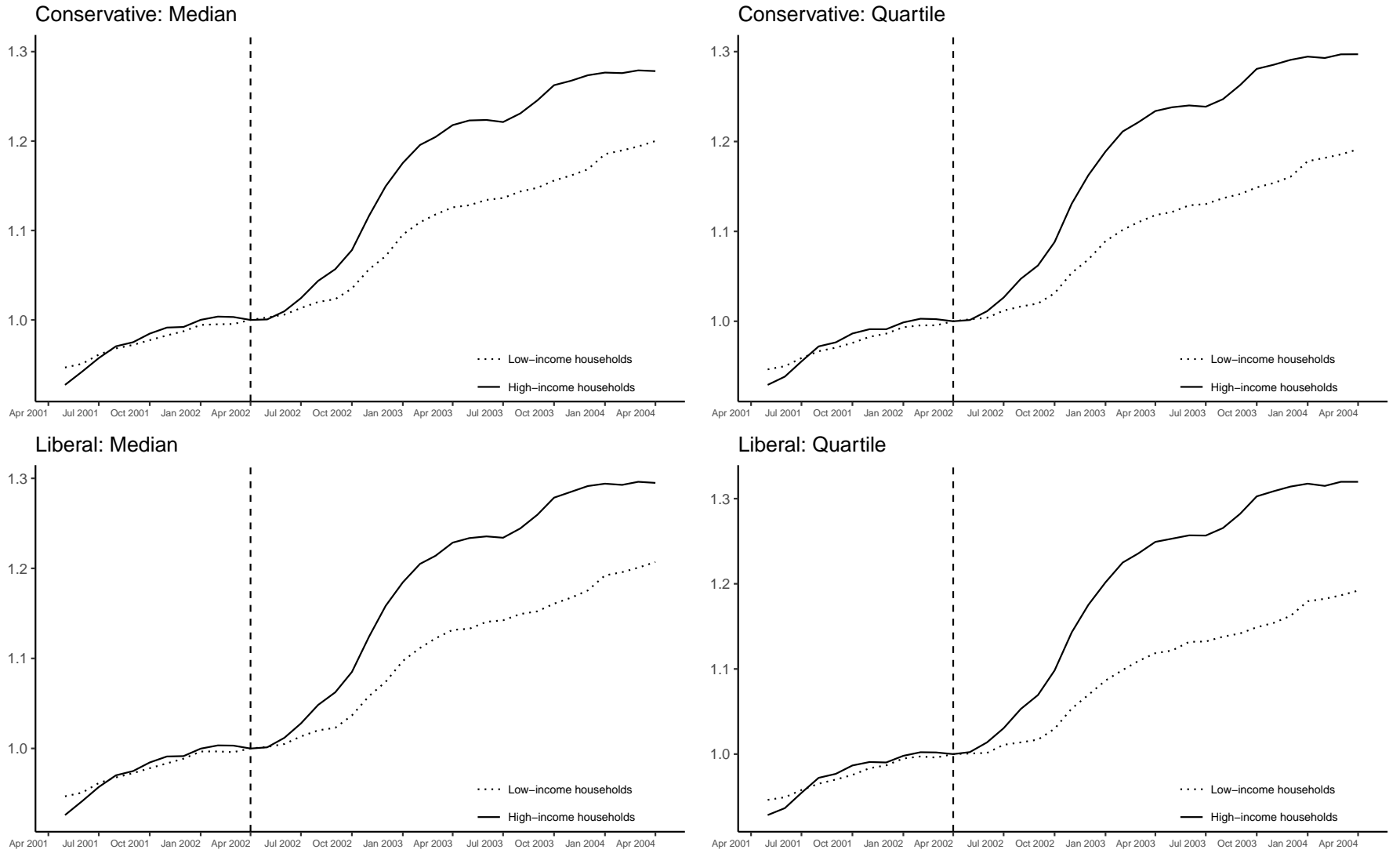
Note: The household-specific across price index is computed as a weighted average of economy-wide price indexes for each product category (P_g) and household-specific expenditure shares(ω_g^h): $\hat{P}_{Across,t}^h \equiv \sum_{g \in G} \omega_g^h \hat{P}_{g,t}$. The household-specific across price index is computed using IPC data at 7-digits (512 subitems).

Figure 1.5: Within price indices



Note: The household-specific within price index is computed as a weighted average of economy-wide expenditure shares (ω_g) and household-specific price indexes for each product category (P_g^h): $\hat{P}_{Within,t}^h \equiv \sum_{g \in G} \omega_g \hat{P}_{g,t}^h$

Figure 1.6: Combined price indices



Note: The household-specific within price index is computed as a weighted average of economy-wide expenditure shares (ω_g) and household-specific price indexes for each product category (P_g^h): $\hat{P}_{Within,t}^h \equiv \sum_{g \in G} \omega_g \hat{P}_{g,t}^h$

Table 1.1: The Across price indices by income decile

	Deciles										Aggregate	Actual CPI	1st/10th ratio
	1	2	3	4	5	6	7	8	9	10			
Panel A: group level													
Brazil													
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.184	1.185	1.182	1.181	1.178	1.176	1.174	1.172	1.169	1.161	1.169	1.168	1.144
2004-04-01	1.245	1.244	1.243	1.243	1.239	1.237	1.235	1.232	1.227	1.220	1.228	1.229	1.118
City of São Paulo													
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.153	1.148	1.150	1.143	1.148	1.148	1.144	1.141	1.138	1.136	1.141	1.145	1.123
2004-04-01	1.197	1.193	1.194	1.190	1.193	1.194	1.191	1.191	1.190	1.190	1.191	1.192	1.034
Panel B: subitem level													
Brazil													
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.252	1.256	1.241	1.239	1.224	1.212	1.205	1.193	1.172	1.150	1.182	1.168	1.682
2004-04-01	1.317	1.321	1.306	1.304	1.286	1.274	1.268	1.252	1.229	1.208	1.242	1.229	1.523
City of São Paulo													
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.180	1.167	1.163	1.147	1.150	1.156	1.142	1.135	1.127	1.117	1.135	1.145	1.536
2004-04-01	1.225	1.211	1.204	1.194	1.202	1.205	1.193	1.195	1.193	1.190	1.196	1.192	1.183

Notes: Column Aggregate refers to the across price index using economy-wide weights. Column CPI reports the actual figures from IPCA-IBGE and IPC-FIPE. Column 1st/10th ratio refers to the accumulated inflation ratio between households in the first (poor) and tenth (rich) deciles. The household-specific across price index is computed as a weighted average of economy-wide price indexes for each product category (P_g) and household-specific expenditure shares (ω_g^h): $\hat{P}_{Across,t}^h \equiv \sum_{g \in G} \omega_g^h \hat{P}_{g,t}$. The household-specific across price index is computed using IPC data at 7-digits (512 subitems). Source: Author's calculation.

Table 1.2: The Within price indices by income decile

	Conservative				Liberal			
	Below Median	Above Median	Quartile 1	Quartile 4	Below Median	Above Median	Quartile 1	Quartile 4
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
2003-04-01	1.155	1.142	1.167	1.130	1.165	1.145	1.182	1.127
2004-04-01	1.228	1.206	1.242	1.193	1.245	1.212	1.267	1.191

Note: The household-specific within price index is computed as a weighted average of economy-wide expenditure shares (ω_g) and household-specific price indexes for each product category (P_g^h): $\hat{P}_{Within,t} \equiv \sum_{g \in G} \omega_g \hat{P}_{g,t}^h$

Table 1.3: The Combined price indices

	Conservative				Liberal			
	Below Median	Above Median	Quartile 1	Quartile 4	Below Median	Above Median	Quartile 1	Quartile 4
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
2003-04-01	1.218	1.126	1.234	1.118	1.229	1.132	1.249	1.119
2004-04-01	1.278	1.200	1.297	1.191	1.295	1.207	1.320	1.192

Note: The household-specific within price index is computed as a weighted average of economy-wide expenditure shares (ω_g) and household-specific price indexes for each product category (P_g^h): $\hat{P}_{Within,t} \equiv \sum_{g \in G} \omega_g \hat{P}_{g,t}^h$

CHAPTER 2

EMPLOYMENT EFFECTS OF REAL EXCHANGE RATE SHOCKS: EVIDENCE FROM BRAZILIAN LOCAL LABOR MARKETS

2.1 Introduction

On August 05, 2019, as the US-China trade war escalated, President Trump tweeted: “*China has always used currency manipulation to steal our businesses and factories, **hurt our jobs**, depress our worker’s wages and harm our farmer’s prices*” (emphasis added by the author). Despite his singular views and opinions on most topics, President Trump is not alone in bringing the exchange rate to the center of the policy debate. Not long ago, in the aftermath of quantitative easing in the US, then Brazilian Finance Minister Guido Mantega stated that “*we’re in the midst of an international currency war, a general weakening of currency*” [Mantega, 2010]. These quotes highlight the fact that the real exchange rate is a key price in open economies and understanding how it affects the economy is crucial for policymakers.

In this paper, I focus on one of the outcomes often singled out by policymakers in the exchange rate debates: jobs. From a theoretical perspective, the response of labor demand to real exchange rate shocks depends on three channels. Following a devaluation, export-oriented firms become more competitive, leading to an increase in demand for domestic

products sold abroad. At the import side, however, the effect is ambiguous. Domestic import-competing sectors may expand as they become more competitive with the increase in the price of foreign goods. Meanwhile, sectors that rely heavily on imported inputs of production may contract because their cost of production increases [Campa and Goldberg, 2001].

To empirically study the employment effects of real exchange rate shocks, I exploit variation in the intensity of real exchange rate shocks across Brazilian local labor markets. I construct two measures of exchange rate shocks to capture the different channels discussed in the previous paragraph. One uses bilateral real exchange rate shocks weighted by sectoral bilateral exports between Brazil and its partners to build a measure of sectoral real exchange rate shocks to export-oriented firms. The other measure uses weights based on sectoral import flows to build a measure of sectoral real exchange rate shocks to import-affected firms due to import competition and the use of imported inputs in production. These sectoral real exchange rate shocks are, then, weighted by the sectoral composition of employment of local labor markets to obtain the intensity of the export and import-weighted real exchange rate shocks at the regional level.

Overall, my main results show positive effects of a devaluation of the export-weighted real exchange rate on total employment. In my preferred specification, a 1 percentage point devaluation of the export-weighted real exchange rate leads to *0.26* percentage point increase in employment on impact (i.e. in the same year of the devaluation). The cumulative effect of the devaluation grows over time reaching *0.8* percentage points four years after the

shock. A 1 percentage point devaluation of the import-weighted real exchange rate does not change employment in the same year of the shock, but there is a negative cumulative effect one year after the shock of 0.25 percentage points. The cumulative effect of this shock does not continue rising as in the export case and is not significant over the medium run (4 years after the devaluation).

Next, I study the effects of real exchange rate shocks at the sectoral level. First, I investigate the response of tradable versus nontradable employment across local labor markets. The results show that the effects I find for total employment are driven by what happens to the tradable sector. Following a 1 percentage point devaluation of the export-weighted exchange rate shock, tradable employment increases by 0.35 and 1.11 percentage points on impact and over the medium run, respectively. For the import-weighted shock, the effect is only significant in the short-run with tradable employment decreasing by 0.32 percentage points. I find no significant effect of either measure of exchange rate shock on nontradable employment. Second, I look within the tradable sector to assess whether the manufacturing and primary sectors respond differently to the real exchange rate shocks. I find no effect of the export-weighted real exchange rate shocks on employment in primary sectors, but a marginally significant negative effect of 0.47 percentage point in the short run for the import-weighted real exchange rate shocks. Manufacturing employment, however, responds to both shocks. In the export side, following a 1 percentage point devaluation, manufacturing employment rises by 0.77 percentage points on impact and reaches

1.76 percentage points over the medium run. In the import side, the input-cost channel predominates and I find a negative effect of 0.44 percentage points over the medium run.

These results are robust to a set of robustness checks. They still hold if we control for possible confounders like the trade liberalization reform of the early 1990s and commodity price shocks. They are also robust to the level of sectoral aggregation or the time periods used to compute the labor shares when building the regional real exchange rate shocks.

This paper is related to the vast literature on the local labor market effects of international economic shocks. Exploiting regional variation in the sectoral composition of employment, studies have shown that local labor markets respond strongly to trade liberalization episodes [Erten et al., 2019, Dix-Carneiro and Kovak, 2017, Hakobyan and McLaren, 2016, Kovak, 2013], import competition from China [Acemoglu et al., 2016, Autor et al., 2013, 2014, Costa et al., 2016] and export demand from China [Feenstra et al., 2019, Costa et al., 2016]. Regional variation has also been used to study the impact of exchange rates on wages [Goldberg and Tracy, 2000] and employment [House et al., 2019] in US states, employment in US metropolitan areas [Huang and Tang, 2016] and employment in Switzerland municipalities [Egger et al., 2017].

Goldberg and Tracy [2000] show that exchange rate movements affect wages of U.S. workers in manufacturing and non-manufacturing industries. The effect of the exchange rate on wages are, however, small when averaged across periods of appreciation and depreciation. These effects are also heterogeneous across workers educational level. Wages of less educated workers decrease more during dollar appreciations and when these workers

have to transition jobs, while more educated workers who remain in the same job experience wage gains during appreciations. House et al. [2019] use differences in trade exposure across U.S. states to study the effects of changes in the exchange rate on economic activity. The authors find that a depreciation in the state-specific trade-weighted real exchange rate increases state exports, reduces state unemployment and increases hours worked.

Exploiting differences in sectoral composition of employment across major US metropolitan areas, Huang and Tang [2016] estimate the effects of the exchange rate on employment. The authors find that a depreciation of the export-weighted real exchange rate has a positive direct effect on manufacturing employment and an indirect effect on local non-manufacturing employment that is increasing with the size of the local manufacturing sector. In metropolitan areas with high concentration of manufacturing employment, this spillover effect is statistically significant and about 60% as large as the direct effect.

Exploiting the exogenous appreciation of the Swiss Franc due to its safe haven status during the financial crisis of 2008, Egger et al. [2017] study how trade-induced shocks across all sectors of the Swiss economy affected municipal employment. Using detailed employment data on the entirety of Swiss firms, the authors find evidence for three channels of employment effects of currency appreciation: negative employment growth induced by increasing export uncompetitiveness and higher import competition, and positive employment growth induced by cheaper availability of foreign inputs. Overall, they find that the combined effect of the three channels on employment growth is negative.

This is the first paper, as far as I know, to use regional variation to study the employment consequences of exchange rate shocks in a developing economy setting. Moreover, none of the papers using regional variation to study the effects of real exchange rate shocks on employment investigate how these effects may vary over time. This is an important contribution as my results show increasing employment effects of real exchange rate shocks on exporting firms with an estimated medium-run impact on total employment more than three times larger than the short-run impact.

The remainder of this chapter is organized as follows. Section 2.2 details the data sources. Section 2.3 details the construction of the measures of exchange rate shocks and describes the difference-in-difference specification used to exploit differences in the intensity of real exchange rate shocks faced by each local labor market in Brazil. The main results are discussed in section 2.4. Section 2.5 concludes.

2.2 Data

2.2.1 Real exchange rate shocks

Real exchange rate (RER) indices come from Penn World Table 9.1 (PWT9.1) [Feenstra et al., 2015]. I use the variable PL_GDP^o , the ratio of nominal GDP in local currency to output-side real GDP as my measure of RER. This measure is equivalent to the country's PPP exchange rate relative to the US dollar and has been used extensively in the exchange rate literature since Rodrik [2008]. Bilateral real exchange rates are computed as the ratio between two countries RER. I normalize all RER indices to 1 in 1995.

2.2.2 Trade data

Export and import bilateral flows are obtained at “The Atlas of Economic Complexity,” Center for International Development at Harvard University, <http://www.atlas.cid.harvard.edu>. “The Atlas” exploits the fact that trade data in the UN Comtrade database is recorded twice – by the importer and the exporter – to cross-reference the records of each country and correct inconsistencies in the data. From “The Atlas”, I obtain bilateral trade flows from 1995 to 2016 classified according to the Harmonized System (HS) at 6 digits.

2.2.3 Employment data

Data on labor market outcomes are from the *Relação Anual de Informações Sociais* (RAIS), an administrative dataset from the Brazilian Ministry of Labor. RAIS provides a yearly census of the Brazilian formal labor market over the period 1986 to 2016. Almost all formally employed workers have information reported in RAIS since this is a requirement for workers to access governments benefits and labor protections. Moreover, firms may face fines for failure to report. Since RAIS is a census rather than a sample, it is representative at fine geographic levels allowing one to study the impacts on local labor markets. Local labor markets are identified as the microregions defined by IBGE – the equivalent of commuting zones in the US – and they are combined when there are boundary changes over the period. I update the definition of local labor markets used in Dix-Carneiro and Kovak [2017] to

include boundary changes that happened after 2010, allowing us to observe 486 local labor markets from 1991 to 2016.

2.2.4 Census controls

I use data from the long form of the 1991 Demographic Census, which contains a 5.8 percent sample of the Brazilian population in 1991, to compute the following control variables: income per capita, the share of the workforce in rural areas, in the informal sector and in the public administration.

2.2.5 Sectoral classification

To calculate the intensity of the real exchange rate shocks at the local labor markets level, I need to have trade and employment data classified according to the same classification. I concord the trade data at HS 6 digits to the International Standard Industrial Classification (ISIC) Rev 3.1 at 4 digits using the concordance table from the World Bank.¹ In the RAIS dataset, we only have sector of employment classified according to the Brazilian ISIC – the *Classificação Nacional de Atividades Econômicas* (CNAE) – starting in 1995. In 2002 the classification is updated to CNAE 1.0 to follow the ISIC Rev 3.1 classification². I use concordance tables from the Brazilian Institute of Geography and Statistics (IBGE)

¹The concordance table is available at the World Integrated Trade solution website.

²The ISIC classification was created by the United Nations Statistics Division in 1948 and has passed through many revisions over the years. CNAE follows the ISIC 3.0 Revision from 1989 while CNAE 1.0 updates the classification to the ISIC 3.1 Revision implemented in 2002.

to concord all employment data according to ISIC Rev 3.1.³ When concurring CNAE and ISIC, I use data at 3 digits to minimize miss classification as there is a significant number of cases with 1 to n matches when working with the data at 4 digits. I also bundle together all non-tradable industries into a single non-tradable sector. The final classification used to build the real exchange rate shocks has 121 sectors.

2.2.6 Sample selection

In the empirical application, I use a baseline sample according to the following criteria. From PWT9.1, I exclude all countries whose price levels are not consistent (flagged as outliers in the database) or whose real exchange rate time series is incomplete. These restrictions leave us with 174 countries for which I can calculate bilateral real exchange rates over the period 1995-2016. From RAIS, I exclude all workers employed by the public administration as the labor market regulations in this sector are very different from the ones of the private sector. I also limit the sample to workers aged 18-64, with a valid individual identifier (similar to the social security number in the US) and with valid information on the sector of employment. In terms of local labor market, for the baseline sample I keep only microregions with positive employment in all three major sectors (primary, manufacturing and nontradable) in all years over the period 1995-2016. The final sample is a balanced panel of 446 microregions observed from 1995 to 2016.⁴

³These concordance tables are available at the IBGE website.

⁴The results do not change qualitatively if we include the 40 excluded microregions in the baseline. But, they get less precise.

2.3 Research design

This paper studies the consequences of RER shocks for employment in Brazilian local labor markets. It does so by exploiting three sources of variation. First, as we can see from figure 2.1, RER shocks varies significantly by trade partner. This fact holds in times of large devaluations, large appreciations or relative stability of the Brazilian Real. Second, the sectoral composition of trade between Brazil and its partners is also heterogeneous. Together, these two sources of variability lead to variation in the average relative price shock facing each sector. Finally, the sectoral composition of employment is not the same for all Brazilian local labor markets. Each local labor market then receives the RER shocks in different intensities.

I construct the local RER shocks as following. For each Brazilian partner p , I compute the bilateral RER shock at time t as $s_t^p = [\ln(RER_t^p) - \ln(RER_{t-1}^p)] \times 100$. Next, I construct two measures of sector-specific shocks for each sector. The export-weighted sector-specific RER shocks are given by:

$$s_{jt}^X = \sum_p \left(\frac{X_{pjt}}{X_{jt}} s_t^p \right) \quad (2.1)$$

and the import-weighted by:

$$s_{jt}^M = \sum_p \left(\frac{M_{pjt}}{M_{jt}} s_t^p \right) \quad (2.2)$$

where j indexes sectors, X and M refer to exports and imports, respectively.

Since the shift-share design exploits differences in the intensity of the real exchange rate shock facing each locality, I construct local-specific shocks using the sectoral composition of labor in each local labor market:

$$S_{lt}^i = \sum_j \frac{L_{lj0}}{L_{l0}} s_{jt}^i \quad (2.3)$$

where l indexes local labor markets and $i = \{X, M\}$ refers, respectively, to the export or import-weighted sectoral RER shocks. To avoid endogeneity, the labor shares used in equation (2.3) are calculated using data from before the RER shocks (then time index equals 0). In the baseline estimations, I compute the labor shares using three-year moving averages ending the year before the shock, *i.e.* using employment data from $t - 3$ to $t - 1$.

Figure 2.2 maps the regional variation of each measure of local RER shocks. It suggests that the incidence of real exchange rate shocks varies significantly across Brazil. Moreover, these maps also highlight the fact that regions facing greater intensity of the export-weighted RER shocks are not the same as the ones facing greater intensity of the import-weighted RER shocks. This is important because it shows the two measures are indeed capturing different sources of variation.⁵

I use the following specification to compare the evolution of employment across local labor markets more and less exposed to real exchange rates shocks:

⁵I also provide a robustness exercise in section 2.4.1.1 that supports this point.

$$y_{lt} = \sum_{\tau=-3}^4 \beta_{\tau} S_{l(t-\tau)}^M + \sum_{\tau=-3}^4 \alpha_{\tau} S_{l(t-\tau)}^X + \gamma X_{l0} + e_{lt} \quad (2.4)$$

where y is equal to a 100 times the log change in employment. β_{τ} 's and α_{τ} 's capture the cumulative effects of the export and import-weighted real exchange rate shocks on the outcome variables. I will refer to the effects up to one year as short run and up to four years as medium run. For example, for the import-weighted real exchange rate shock, $\beta_0 + \beta_1$ is the short-run effect and $\beta_0 + \dots + \beta_4$ is the medium-run effect. To address the potential concern that any results may simply represent a continuation of local labor market trends, I use the leading coefficients of the exchange rate shocks variables to assess the existence of pre-trends. I also include a set of start-of-period controls and time fixed effects (captured by the term X_{l0}).

In addition to microregions, IBGE also defines mesoregions which are higher level geographic units based on measures of local market integration. I, then, include mesoregion fixed-effects in my preferred specification so as to check whether the results are robust to accounting for contemporaneous mesoregion-specific trends in employment levels. In order to allow for spatial correlation of errors across microregions, I cluster standard errors at the level of the mesoregion. After accounting for border changes over time, there are 114 mesoregions. I also use an alternative specification with state-specific trends and standard

errors clustered at the state level (there are 26 states and a federal district in Brazil). The results are qualitatively the same of the specification using mesoregion.⁶

2.4 Results

This section discusses the main results of the paper. I begin by showing the response of total local employment to real exchange rate shocks. I then investigate the effects on sectoral employment. First, I distinguish between the tradable and nontradable sectors. Later, I look within the tradable sector and assess the differential impact of exchange rate shocks on the manufacturing and primary sectors.

2.4.1 Total Employment

Table 2.1 reports results for the effects of real exchange rate shocks on total employment. All specifications contain year fixed effects. The second column adds state-specific trends and the third adds a set of demographic controls (income per capita, the share of the workforce in rural areas, in the informal sector and in the public sector) together with the state-specific trends. Both specifications cluster standard errors at the state level.

⁶Adão et al. [2019] show that the usual inference procedures overreject the null hypothesis in shift-share regressions. The reason is that the residual in shift-share regressions is likely to be correlated across regions with similar sectoral composition, independently of their geographic location. The authors, then, provide two inference procedures to correct the overrejection problem. Similarly, Borusyak et al. [2020] provide an equivalence result showing that the shift-share regressions coefficients can be obtained from regressions at the level of the shocks. The authors also show that estimating the coefficients at the level of identifying variation (the shock-level regressions) can yield asymptotically valid standard errors. However, the codes available to implement both procedures do not work directly with the distributed lag specification with two shift-share variables as in my case. I, then, rely on clustered-standard errors estimators based on results provided by Adão et al. [2019] showing that the overrejection problem is more severe with small number of sectors. This problem should, then, be attenuated in my baseline results as they are obtained using 121 sectors.

Columns (4) and (5) proceed the same way, but replace states with mesoregions as the higher geographic level. Overall, the results show positive impacts of export-weighted real exchange rate shocks on total employment on impact and over the medium run, while import-weighted real exchange rate shocks show negative impacts limited to the short run. Moreover, the estimated coefficients are very stable across all specifications.

Figure 2.3 presents the impulse response of total employment obtained with the estimated coefficients of column (5), which is my preferred specification. A 1 percentage point devaluation of the export-weighted real exchange rate leads to 0.26 percentage point increase in employment on impact (i.e. in the same year of the devaluation). The cumulative effect of the devaluation grows over time reaching 0.8 percentage points four years after the shock. A 1 percentage point devaluation of the import-weighted real exchange rate does not change employment in the same year of the shock, but there is a negative cumulative effect one year after the shock of 0.25 percentage points. The cumulative effect of this shock does not continue rising as in the export case and is not significant over the medium run (4 years after the devaluation). Results would be very similar if the impulse responses were computed with coefficients from column (3), where states, instead of mesoregions, are used for controlling for specific trends and clustering standard errors. Figure 2.3 shows no sign of pre-trends for the import-weighted shock, but the size of the cumulative effect of the export-weighted shock up to $t - 2$ is of potential concern. It is important to highlight, however, that it does not seem likely that the effect of the export-weighted exchange rate

shock is just a continuation of pre-trends as the pre-trends does not move in the same direction of the short and medium-run effects.

2.4.1.1 Robustness

In this subsection, I provide additional results to show that the baseline employment effects I find are not driven by other possible confounding shocks to Brazilian local labor markets and are also robust to alternative measurement and specification choices.

In the early 1990s, Brazil implemented a significant trade liberalization reform. As shown by Dix-Carneiro and Kovak [2017], the impact of the tariff changes from 1990-1995 on regional formal employment and earnings were significant. Regions facing larger tariff cuts experienced prolonged decreases in formal employment and earnings. Therefore, it is possible that the tariff changes during the liberalization reform may be correlated with the exchange rate shocks, especially if they have affected the local sectoral composition of employment. Another possible confounding shock was the global commodity price boom of the late 2000s, given that the evolution of commodity prices are important for the evolution of exchange rates, especially in developing economies. Table 2.2 reports results for my preferred specifications where I add regional measures of the trade liberalization reform from Dix-Carneiro and Kovak [2017] and regional commodity prices from Adao [2016]. The trade liberalization shock do not vary over time, while the commodity prices are available from 1991 to 2010. Therefore, when the commodity price control is included the results are obtained using a shorter panel over 1995-2010. Despite the different sample

periods, the employment effects are robust to the inclusion of both confounders and the estimated coefficients are very similar to the ones obtained in the baseline estimations.

Even though I have shown in Figure 2.2 that regions most affected by the export-weighted and import-weighted real exchange rate shocks are not the same, a possible concern when including both measures in the same specification is that they may be capturing the same type of variation. Table B.1 presents results of specifications in which only one of the two measures is included in each regression. Overall, these results are very similar to baseline, with the only difference that the impact of the import-weighted real exchange rate shock when included alone last longer and is significant four years after the shock.

As a final set of robustness tests, Appendix B presents results using different criteria to build the local real exchange rate shocks. In Table B.2, I report results obtained by aggregating the sectoral classification at 2 digits which consists of 31 sectors. In Table B.3, I report results obtained when labor shares are computed using data only from $t - 1$ or $t - 2$. In both cases, the results are qualitatively the same.

2.4.2 Sectoral Employment

The previous section presented evidence that real exchange rate shocks increase total local employment through an exporting channel in the short and medium run, while they decrease employment through an importing channel only in the short run. Now, I show how real exchange rate shock effects on employment vary by sectors of the local economy. In addition to affecting the tradable sector, real exchange rate shocks may also change

employment in the non-tradable sector as they change the relative price of non-tradable goods and services. The expected effect on employment in the non-tradable sector is ambiguous, as it depends on the relative strength of the income and substitution effects. Moreover, it is also possible that there is heterogeneous effects within the tradable sector and, therefore, I investigate how employment in manufacturing and primary industries responds to the exchange rate shocks.

Table 2.3 report the results for employment in the tradable and non-tradable sectors. It shows that the effects I find for total local employment are driven by what happens to employment in the tradable sector. Following a 1 percentage point devaluation of the export-weighted exchange rate shock, tradable employment increases by 0.35 and 1.11 percentage points on impact and over the medium run, respectively. For the import-weighted shock, the effect is only significant in the short-run with tradable employment decreasing by 0.32 percentage points. Reassuringly, Figure 2.4 shows no signs of pre-trends for the tradable sector estimates. It also shows a slight pre-trend/anticipation effect for the non-tradable sector, but in this case I find no significant effect of either measure of exchange rate shock on nontradable employment.

In Table 2.4, I break down employment in the tradable sector into manufacturing and primary. I find no effect of export-weighted real exchange rate shocks on employment in primary sectors. In the short run, there is a marginally significant (at the 10% level) negative effect of 0.47 percentage point for the import-weighted real exchange rate shock, but this negative cumulative effect fades away and is not significant three years after the

devaluation as shown in Figure 2.4. Manufacturing employment, however, responds to both shocks. In the export side, following a 1 percentage point devaluation, manufacturing employment rises by 0.77 percentage points on impact and reaches 1.76 percentage points over the medium run. In the import side, the input-cost channel predominates and I find a negative effect of 0.44 percentage points over the medium run. Similarly, to the total employment case, Figure 2.4 shows some pre-trends but now only for the import-weighted shock. The primary sector is less concerning as the pre-trend does not go in the same direction of the main effects, which are also not significant over the medium run as highlighted before. However, the manufacturing case requires more caution given that the pre-trend move in the same direction of the main effects.

2.4.2.1 Robustness

This section reports robustness tests for the sectoral employment estimations. The sectoral employment effects of real exchange rate shocks are qualitatively the same when controlling for confounders like the trade liberalization of early 1990s and commodity price shocks as shown in Tables B.4, B.5, B.6 and B.7. Table B.8 shows that the size of the effects on manufacturing and primary sectors employment are slightly sensitive to the level of aggregation used to build the regional exchange rate shocks. The negative employment effects on the manufacturing sector of import-weighted real exchange rate shocks are significantly higher in the medium run when regional shocks are computed using 2 digits compared to 3 digits. The positive effects on primary sector employment of export-weighted real ex-

change rate shocks are also significantly higher at 2 digits compared to 3 digits, when they were not even statistically significant. Similar to the total employment case, the sectoral employment effects are, however, robust to labor shares being calculated with data from $t - 1$ or $t - 2$ as reported in Tables B.9 and B.10.

2.5 Conclusion

To study the impact of the real exchange rate on employment in Brazil, this paper exploits variation in the intensity of real exchange rate shocks across Brazilian local labor markets over the 1995-2016 period. I build two measures of the real exchange rate shocks to capture the export and import channels.

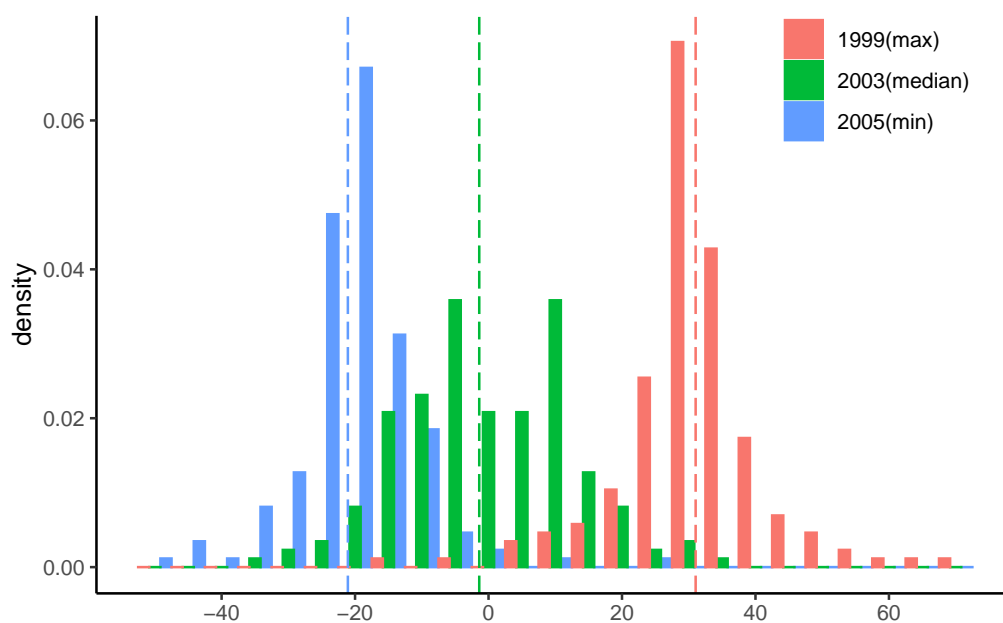
I find that a 1 percentage point devaluation of the export-weighted real exchange rate leads to 0.26 percentage point increase in employment on impact and reaches 0.8 percentage points four years after the shock. Meanwhile, a 1 percentage point devaluation of the import-weighted real exchange rate leads to a 0.25 percentage point decrease in employment in the short run. The cumulative effect over the medium run of this shock is, however, not significant.

I then explore how these effects vary by sector. My results suggest that the effects on total employment are explained by the response of the tradable sector, as I find no significant effect of either measure of the real exchange rate shock on nontradable employment. Tradable employment increases by 0.35 on impact and 1.11 percentage points over the medium run after a 1 percentage point devaluation of the export-weighted real exchange

rate shock; and it decreases 0.32 percentage points after a shock of the same size of the import-weighted real exchange rate. Within the tradable sector, employment in primary sectors is not affected through the export channel while the short-run negative effect of 0.47 percentage point of the import-weighted shock is only marginally significant. Manufacturing employment, however, responds to both shocks. Manufacturing employment rises by 0.77 percentage points on impact and reaches 1.76 percentage points over the medium run after a 1 percentage point devaluation of the export-weighted real exchange rate; and it decreases 0.44 percentage points over the medium run after a shock of the same of the import-weighted real exchange rate.

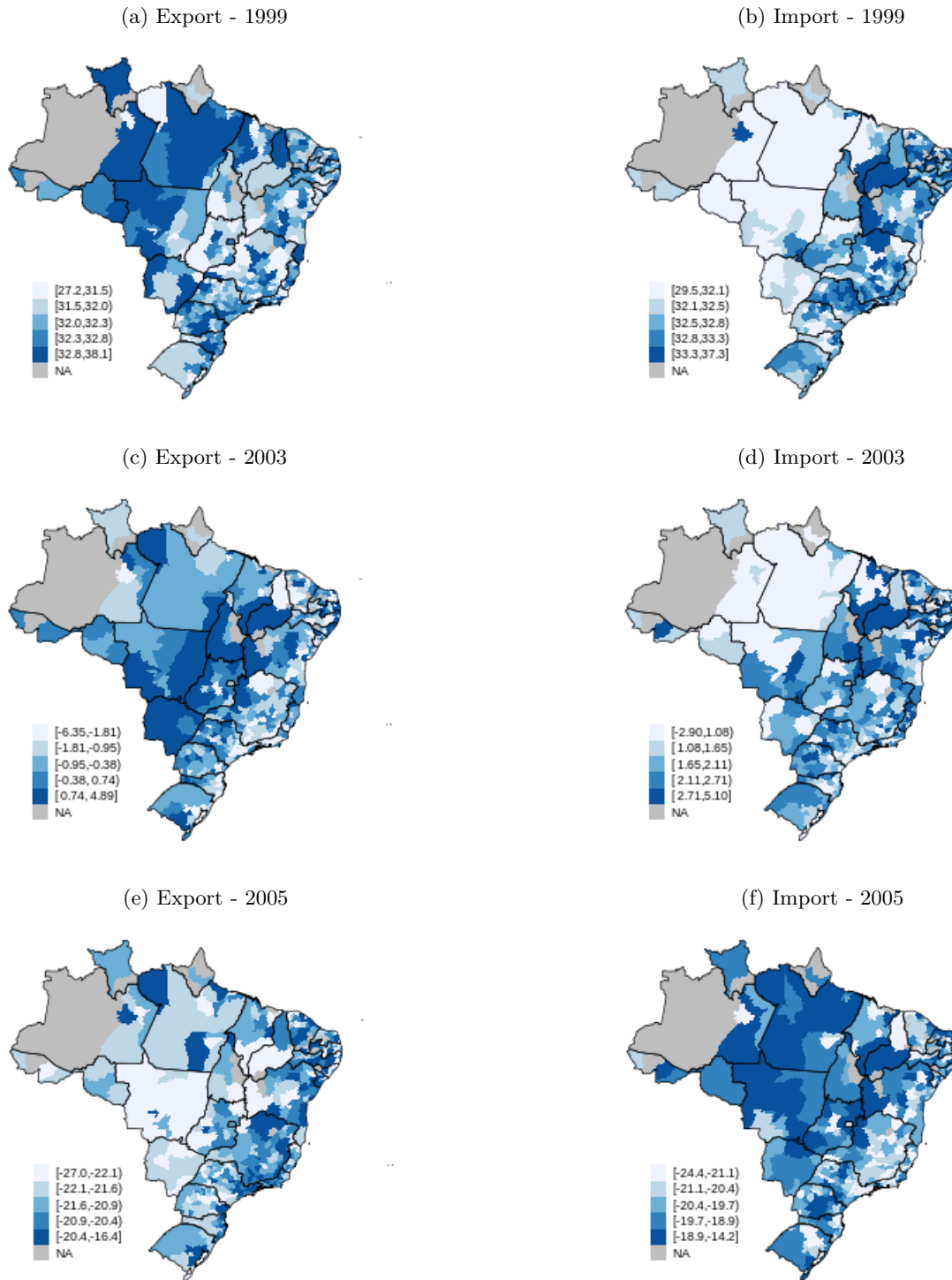
FIGURES AND TABLES

Figure 2.1: Distribution of bilateral real exchange rate shocks



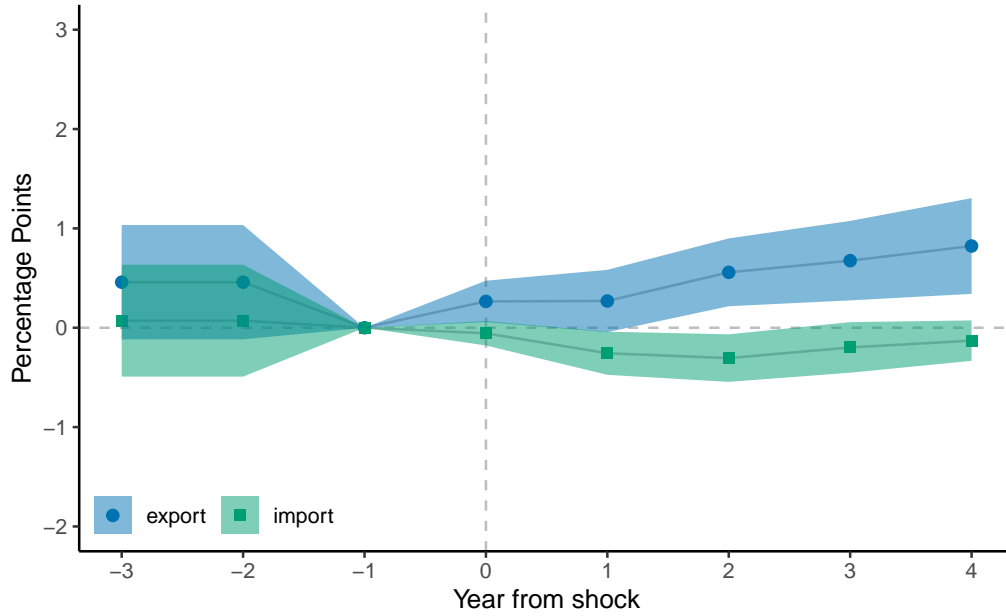
Notes: Bilateral real exchange rate shocks are defined as 100 times the log change of bilateral real exchange rate between Brazil and its trade partners. Distributions for years of a large devaluation (max), a large appreciation (min) and relative stability (median) defined according to median bilateral shock for each year. Dashed lines highlight the median bilateral shock of each year. Source: PWT 9.1.

Figure 2.2: Intensity of regional real exchange rate shocks in 1999, 2003 and 2005 by quintile



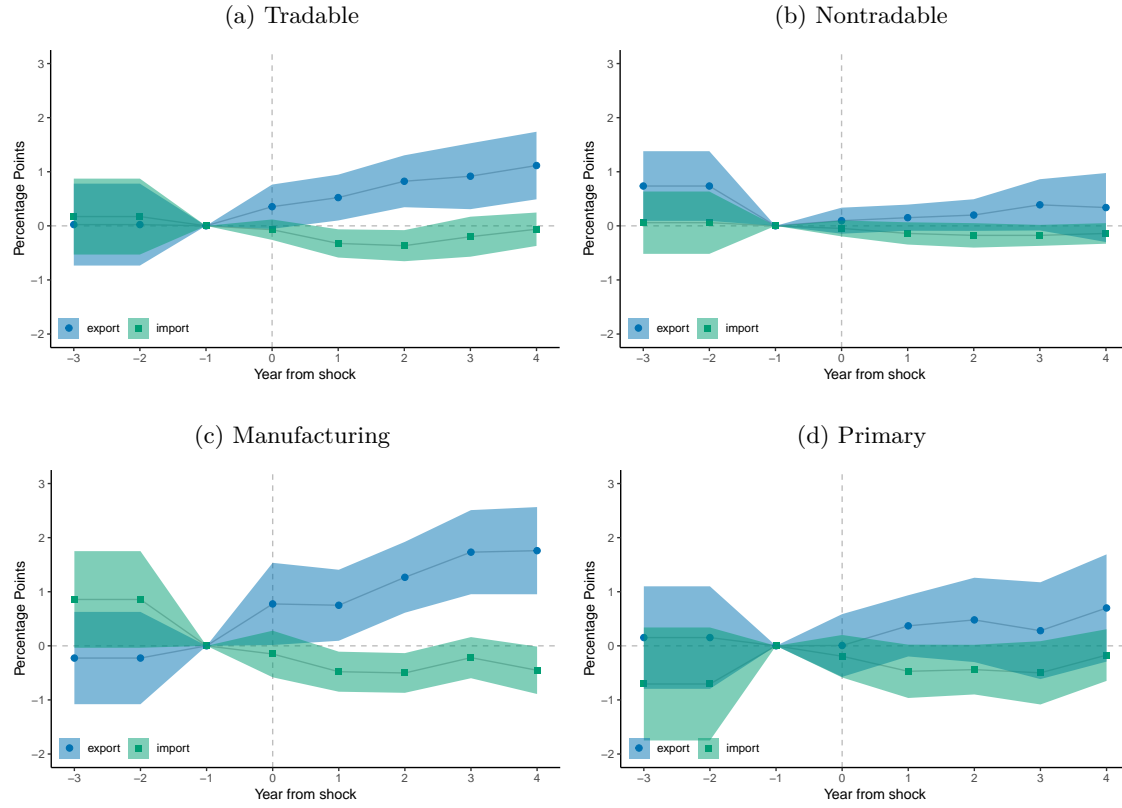
Notes: Regional real exchange rate shocks given by equation 2.3.

Figure 2.3: Cumulative impulse response of employment to exchange rate shocks



Notes: Figure shows cumulative effects from regressions of total formal employment growth on real exchange rate shocks. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by exports or imports of each Brazilian trade partner and the sectoral composition of employment. Coefficients on lags and leads are (separately) summed cumulatively from time -1, where the effect is normalized to 0. The sums of lags include the contemporaneous effect at time 0, which is the contemporaneous shock. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration obtained from the 1991 Census. The shaded area is associated with the 95% confidence interval computed using robust-clustered standard errors at the mesoregion level. Estimates correspond to column (5) of Table 2.1.

Figure 2.4: Cumulative impulse response of sectoral employment to exchange rate shocks



Notes: Figure shows cumulative effects from regressions of formal sectoral employment growth on real exchange rate shocks. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by exports or imports of each Brazilian trade partner and the sectoral composition of employment. Coefficients on lags and leads are (separately) summed cumulatively from time -1, where the effect is normalized to 0. The sums of lags include the contemporaneous effect at time 0, which is the contemporaneous shock. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration obtained from the 1991 Census. The shaded area is associated with the 95% confidence interval computed using robust-clustered standard errors at the mesoregion level. For each sector, estimates correspond to column (5) of Tables 2.3 and 2.4.

Table 2.1: Employment effects of real exchange rate shocks

	Change in employment				
	(1)	(2)	(3)	(4)	(5)
Export-weighted RER					
Contemporaneous effect	0.271*** (0.084)	0.255** (0.127)	0.260** (0.128)	0.260** (0.105)	0.265** (0.106)
Short-run effect	0.283*** (0.100)	0.254 (0.232)	0.264 (0.234)	0.262 (0.160)	0.269* (0.160)
Medium-run effect	0.866*** (0.156)	0.760*** (0.212)	0.774*** (0.227)	0.809*** (0.237)	0.823*** (0.246)
Import-weighted RER					
Contemporaneous effect	-0.047 (0.044)	-0.037 (0.058)	-0.039 (0.058)	-0.054 (0.062)	-0.056 (0.063)
Short-run effect	-0.241*** (0.061)	-0.223* (0.129)	-0.227* (0.129)	-0.254** (0.110)	-0.257** (0.111)
Medium-run effect	-0.079 (0.095)	-0.048 (0.126)	-0.054 (0.125)	-0.124 (0.104)	-0.130 (0.104)
Year fixed effect	Y	Y	Y	Y	Y
State fixed effect		Y	Y		
Meso-region fixed effect				Y	Y
Controls			Y		Y
SE clustered by state		Y	Y		
SE clustered by meso-region				Y	Y

Notes: The dependent variable is measured as 100 times the log change of total formal employment. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by sectoral exports or imports of each Brazilian trade partner and the sectoral composition of employment at the local labor market. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration in the 1991 Census. Clustered-robust standard-errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2.2: Employment effects of real exchange rate shocks controlling for confounding shocks

	Change in employment							
	Baseline				Baseline + Confounders			
	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)
Export-weighted RER								
Contemporaneous effect	0.260** (0.128)	0.265** (0.106)	0.290** (0.128)	0.312*** (0.109)	0.260** (0.130)	0.267** (0.107)	0.288** (0.129)	0.314*** (0.110)
Short-run effect	0.264 (0.234)	0.269* (0.160)	0.354 (0.220)	0.391*** (0.148)	0.262 (0.237)	0.273* (0.161)	0.351 (0.224)	0.394*** (0.149)
Medium-run effect	0.774*** (0.227)	0.823*** (0.246)	0.869*** (0.237)	0.985*** (0.263)	0.770*** (0.228)	0.830*** (0.248)	0.861*** (0.233)	0.992*** (0.263)
Import-weighted RER								
Contemporaneous effect	-0.039 (0.058)	-0.056 (0.063)	-0.019 (0.057)	-0.047 (0.066)	-0.038 (0.058)	-0.057 (0.063)	-0.018 (0.057)	-0.048 (0.066)
Short-run effect	-0.227* (0.129)	-0.257** (0.111)	-0.191 (0.126)	-0.244** (0.113)	-0.226* (0.128)	-0.259** (0.111)	-0.189 (0.125)	-0.245** (0.113)
Medium-run effect	-0.054 (0.125)	-0.130 (0.104)	0.021 (0.120)	-0.105 (0.115)	-0.052 (0.125)	-0.134 (0.103)	0.025 (0.123)	-0.108 (0.114)
Confounders								
Commodity price shocks			1.765 (1.140)	3.788** (1.717)			1.794 (1.141)	3.756** (1.708)
Trade liberalization shock					-2.591 (10.002)	6.752 (7.003)	-4.095 (10.938)	5.475 (8.169)
Year fixed effect	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effect	Y		Y		Y		Y	
Meso-region fixed effect		Y		Y		Y		Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
SE clustered by state	Y		Y		Y		Y	
SE clustered by meso-region		Y		Y		Y		Y

Notes: The dependent variable is measured as 100 times the log change of total formal employment. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by sectoral exports or imports of each Brazilian trade partner and the sectoral composition of employment at the local labor market. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration in the 1991 Census. The commodity price and trade liberalization shock controls are from Adao [2016] and Dix-Carneiro and Kovak [2017] Clustered-robust standard-errors in parentheses.

*** p < 0.01, ** p < 0.5, * p < 0.10.

Table 2.3: Sectoral employment effects of real exchange rate shocks: tradable and nontradable sectors

	Tradable					Non Tradable				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Export-weighted RER										
Contemporaneous effect	0.336*** (0.109)	0.338 (0.226)	0.342 (0.227)	0.356* (0.208)	0.354* (0.209)	0.143 (0.104)	0.108 (0.127)	0.106 (0.127)	0.096 (0.121)	0.095 (0.123)
Short-run effect	0.495*** (0.130)	0.498* (0.256)	0.506* (0.262)	0.527** (0.213)	0.523** (0.216)	0.239* (0.124)	0.178 (0.156)	0.174 (0.154)	0.152 (0.124)	0.151 (0.123)
Medium-run effect	1.065*** (0.203)	1.039*** (0.334)	1.044*** (0.353)	1.132*** (0.306)	1.115*** (0.319)	0.580*** (0.193)	0.392 (0.242)	0.374 (0.252)	0.348 (0.315)	0.338 (0.326)
Import-weighted RER										
Contemporaneous effect	-0.057 (0.058)	-0.050 (0.118)	-0.051 (0.118)	-0.073 (0.096)	-0.071 (0.096)	-0.067 (0.055)	-0.048 (0.060)	-0.046 (0.060)	-0.049 (0.076)	-0.048 (0.075)
Short-run effect	-0.299*** (0.079)	-0.287* (0.158)	-0.289* (0.158)	-0.328** (0.132)	-0.325** (0.133)	-0.172** (0.075)	-0.139 (0.096)	-0.136 (0.096)	-0.143 (0.105)	-0.140 (0.105)
Medium-run effect	0.002 (0.123)	0.025 (0.176)	0.022 (0.172)	-0.069 (0.157)	-0.062 (0.158)	-0.195* (0.117)	-0.131 (0.095)	-0.124 (0.094)	-0.148 (0.096)	-0.140 (0.097)
Year fixed effect	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effect		Y	Y				Y	Y		
Meso-region fixed effect				Y	Y				Y	Y
Controls			Y		Y			Y		Y
SE clustered by state		Y	Y				Y	Y		
SE clustered by meso-region				Y	Y				Y	Y

Notes: The dependent variable is measured as 100 times the log change of formal employment in each sector. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by exports or imports of each Brazilian trade partner and the sectoral composition of employment. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration. Clustered-robust standard-errors in parentheses.

*** $p < 0.01$, ** $p < 0.5$, * $p < 0.10$.

Table 2.4: Sectoral employment effects of real exchange rate shocks: manufacturing and primary sectors

	Manufacturing					Primary				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Export-weighted RER										
Contemporaneous effect	0.753*** (0.182)	0.748* (0.424)	0.753* (0.425)	0.781** (0.387)	0.775** (0.388)	0.029 (0.200)	-0.011 (0.273)	-0.020 (0.274)	0.009 (0.295)	0.006 (0.296)
Short-run effect	0.726*** (0.217)	0.717* (0.373)	0.724* (0.376)	0.762** (0.334)	0.750** (0.335)	0.398* (0.239)	0.332* (0.201)	0.318 (0.204)	0.375 (0.284)	0.369 (0.289)
Medium-run effect	1.702*** (0.339)	1.662*** (0.406)	1.665*** (0.417)	1.794*** (0.409)	1.759*** (0.411)	0.872** (0.373)	0.641 (0.407)	0.596 (0.417)	0.720 (0.495)	0.700 (0.506)
Import-weighted RER										
Contemporaneous effect	-0.155 (0.096)	-0.146 (0.243)	-0.147 (0.242)	-0.155 (0.221)	-0.151 (0.220)	-0.171 (0.106)	-0.154 (0.184)	-0.151 (0.186)	-0.194 (0.199)	-0.192 (0.201)
Short-run effect	-0.486*** (0.132)	-0.470** (0.237)	-0.473** (0.235)	-0.484** (0.191)	-0.478** (0.189)	-0.426*** (0.145)	-0.398 (0.266)	-0.395 (0.270)	-0.473* (0.249)	-0.471* (0.252)
Medium-run effect	-0.470** (0.206)	-0.438 (0.270)	-0.444* (0.265)	-0.465** (0.228)	-0.452** (0.224)	-0.038 (0.226)	0.013 (0.242)	0.018 (0.245)	-0.175 (0.242)	-0.171 (0.244)
Year fixed effect	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effect		Y	Y				Y	Y		
Meso-region fixed effect				Y	Y				Y	Y
Controls			Y		Y			Y		Y
SE clustered by state		Y	Y				Y	Y		
SE clustered by meso-region				Y	Y				Y	Y

Notes: The dependent variable is measured as 100 times the log change of formal employment in each sector. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by exports or imports of each Brazilian trade partner and the sectoral composition of employment. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration. Clustered-robust standard-errors in parentheses.

*** $p < 0.01$, ** $p < 0.5$, * $p < 0.10$.

CHAPTER 3

PARTISANSHIP AND LOCAL FISCAL POLICY: EVIDENCE FROM BRAZILIAN CITIES

3.1 Introduction

Do political parties matter when it comes to governing cities? Despite competitive pressures for platform convergence [Downs, 1957], policy differences can emerge when parties are ideologically motivated and represent different constituencies [Alesina, 1988]. While these broad theoretical considerations apply to all government levels, municipalities present some specificities. Cities tend to be subject to more intense fiscal competition, stronger spatial sorting and tighter financial constraints, relative to higher levels of government [Ferreira and Gyourko, 2009, pp. 401-403]. Moreover, the set of issues that are relevant for city governments is generally of a different nature and possibly less subject to partisan divide. It is therefore an open question whether the substantial degree of policy divergence that is often observed at the national and regional level may be observed in municipal governments as well. A recent literature has studied this issue empirically in the US and other industrialized countries, finding that partisan control of a city government has some effect on local policies and outcomes in some European nations [Pettersson-Lidbom, 2008, Fiva

et al., 2016], while evidence from the US is mixed [de Benedictis-Kessner and Warshaw, 2016, Gerber and Hopkins, 2011, Ferreira and Gyourko, 2009].

In the context of developing economies and young democracies, theoretical predictions are even less clear-cut, and empirical evidence is lacking. A widespread view holds that local politics in these contexts tends to be dominated by patronage and personalistic attitudes, while weak parties seldom develop distinctive policy platforms and programmatic linkages to voters.¹

This paper studies partisan effects in Brazilian cities. We estimate the effect of electing a left-wing mayor on municipal fiscal policy, using a regression-discontinuity design. We then test the role of re-election concerns, Tiebout competition, institutional constraints and the ideological composition of mayoral coalitions in determining the degree of policy divergence. We focus on mayors, rather than city councils, because in Brazilian cities the executive branch has a dominant role in crafting, approving and executing the municipal budget.

Overall, our baseline results point to substantial (but not complete) fiscal policy convergence between political parties in Brazilian cities. We find no effect of left-wing mayors on the size of the city government nor on the allocation of spending across the main budget categories (current spending, investment and personnel). We do find a modest but robust positive effect on the share of social expenditures. The (close) election of a left-wing mayor

¹We will discuss these issues and how they relate to our results in Sections 3.7 and 3.8.

tends to raise the share of social expenditures by around 0.6 percentage points in our preferred RD specification.

We then explore potential mechanisms which may limit partisan effects in Brazilian cities. Mainstream parties may just not have fundamentally different ideological views when it comes to local fiscal policy. Or they may have different ideological views on this matter, but their policy space may be constrained. Previous literature and our reading of the institutional context suggest four possible types of constraints. The first is re-election concerns, consistent with models of Downsian competition with reputation-building (eg Enelow and Munger [1993], Besley and Case [1995a]). The second is Tiebout-type competition among local jurisdictions, which previous studies have found to be important in bringing about policy convergence in US municipalities [Ferreira and Gyourko, 2009]. The third is institutional rigidities regarding the tax revenue system and the allocation of public expenditures. The fourth is the pre-electoral coalition-building process, which could lead to internally heterogeneous (and therefore ideologically ambiguous) multi-party coalitions in support of mayoral candidates.²

We propose empirical tests for these explanations. To assess the role of re-election concerns, we restrict the analysis to ‘lame-duck’ mayors, who face a binding term limit and are therefore less constrained by electoral competition in pursuing their agenda. Following the US literature [Ferreira and Gyourko, 2009], we test the ‘Tiebout-competition’ hypothesis

²We thank an anonymous referee for suggesting this fourth hypothesis.

by building a Herfindahl index, measuring the presence of potentially competing locations in the same local area, and test whether the impact of partisanship covaries with this index. To test the ‘institutional constraints’ hypothesis, we exploit exogenous changes in these constraints provided by oil windfalls. In Brazil, a subset of oil-producing municipalities experience sharp fluctuations in revenues due to fluctuations in oil production and prices. If partisan effects are limited by institutional constraints, we would expect to find larger effects when these constraints are relaxed by oil-related revenue windfalls. Finally, to test whether internally heterogeneous mayoral coalitions drive policy convergence, we test whether the impact of partisanship covaries with the ideological distance between the winning coalition and its opposition.

Our results suggest that none of these mechanisms explain the lack of partisan effects on the size of government. This suggests limited ideological differences between mainstream parties on this topic. This interpretation appears consistent with both survey evidence on the policy preferences of Brazilian politicians [Cesar Zucco and Power, 2021] and studies of the evolution of the policy proposals of the Workers’ Party (PT), the leading Brazilian left party [Campello, 2016].

However, institutional constraints and re-election concerns *do* appear to explain the limited extent of budget composition effects. In cities where institutional constraints are relaxed by oil windfalls, left-wing mayors raise the share of social expenditures by around 2.2 percentage points, a more than threefold increase compared to the baseline results. We also find a larger effect on the share of social expenditures among ‘lame-duck’ may-

ors (around 1.3 percentage points versus 0.6 in the baseline). We find little support for explanations based on Tiebout-competition or ideologically heterogeneous coalitions.

The budget composition effects we find translate into changes in social expenditures per capita. The close election of a left-wing mayor increases social expenditures per capita by around 1 percent in the baseline sample, by around 3 percent in cities with a ‘lame-duck’ mayor and by more than 6 percent in cities experiencing oil windfalls.

3.1.1 Related literature

Some recent papers have used regression-discontinuity designs (RDDs) to study the causal effect of political partisanship on city-level fiscal policy and other outcomes in high-income countries. These studies have pointed to significant effects of left-wing parties on the size and composition of the city budget in Nordic European countries (Norway and Sweden), while the evidence is mixed for Democrat (as opposed to Republican) mayors in US cities. Little evidence has been available so far on developing countries.³

Specifically, Pettersson-Lidbom [2008] finds that left-wing city governments in Sweden increase the municipal budget, employ more workers, and reduce the local unemployment rate, relative to conservative ones. Folke [2014] adapts the regression-discontinuity framework to study the role of small parties in proportional representation systems, finding

³Most previous work on partisan effects on local policy in developing countries lacks a clear identification strategy, as the one provided by a RDD. It is hard, therefore, to discern causal partisan impacts from selection effects in these previous works. For Brazil, there are a few studies using panel regression with party dummies to study the relation between partisanship and fiscal policy at the local level [Sakurai, 2009, Sakurai and Menezes-filho, 2011, Sakurai and Gremaud, 2007].

large effects of party representation in Swedish municipal councils on immigration and environmental policy, but not taxes.

Fiva et al. [2016] estimate the effect of both government control and party representation in Norwegian cities. They find that a conservative city government lowers property taxes, but has no impact on spending allocations, and that an increase in the seats of left-wing parties leads to higher childcare spending and lower elderly care spending.

Studies of US cities provide a nuanced picture. Overall, they appear to point to null or very limited effects in small and medium-sized municipalities, but more substantial impacts in large cities. Specifically, Ferreira and Gyourko [2009] find no partisan differences in policy outcomes between Democrat and Republican mayors. They investigate possible explanations, and find most support for Tiebout-competition among municipalities within metropolitan areas. Similarly, Gerber and Hopkins [2011] find no major effects on policy outcomes in areas characterized by shared authority among different levels of government. They find, however, that Democrat mayors spend a smaller share of their budget on public safety, an area where mayors have higher sway. Differently from the previous two studies and focusing on larger cities (with more than 75,000 inhabitants), de Benedictis-Kessner and Warshaw [2016] find that electing a Democrat mayor leads to higher expenditures, which are financed by increased indebtedness, with no changes in revenues. These studies

of US cities are the closest to our paper, in the sense of studying a majoritarian system in which a directly-elected mayor is the head of the city government.⁴

Ours is the first study to provide causally identified evidence about the influence of political partisanship on local fiscal policy in the context of a developing country. Moreover, we provide novel evidence on the important role of institutional constraints and re-election concerns in limiting policy divergence.

3.2 Institutional and Political Context

3.2.1 Institutional framework

Brazil is a federal republic with three autonomous and independent administrative levels: the federal government, 27 states (including the federal district) and 5,570 municipalities. Brazilian municipalities have an executive and a legislative branch. The mayor is directly elected by plurality or majority rule and the city council by proportional rule. Local elections happen every 4 years in October and the elected mayor and city council start their mandate in January 1st of the following year. Municipal elections are always

⁴A broader literature has studied partisan effects at the regional and national level on various outcomes. For example, Lee et al. [2004] use close US congressional elections to show that voters merely elect (rather than affecting) candidates' policy positions. Leigh [2008] studies US States in the 1941-2002 period and finds partisan effects on post-tax inequality, unemployment, incarceration rates, minimum wages and welfare caseloads, but no impact on taxes, public employment and crime rates. Beland and Oloomi [2017] study the effect of the party affiliation of US Governors on fiscal policy, finding no effect on total spending but Democratic governors allocating a larger share to health and education. In a related study, Beland [2015] finds that Democratic governors tend to cause reductions in racial gaps in employment and earnings. Two recent studies have focused on US counties, finding relevant partisan effects on their fiscal policy, with Democratic legislators spending more [Benedictis-Kessner and Warshaw, 2020], but no effect of sheriffs' partisanship on their law enforcement behavior [Thompson, 2020]. Other studies have looked at the effect of partisan victories in national elections on financial markets (eg Snowberg et al. [2007]; Girardi [2020]).

two years apart from federal and state elections, which happen at the same time. In municipalities with fewer than 200,000 voters, there is only one round for electing the mayor. In larger cities, there is a runoff between the two most voted candidates if none of them achieves an absolute majority in the first round. Mayors face a two-terms limit.

Importantly for our research design, in Brazilian municipalities (as well as at the federal and state level) the executive branch has a dominant role in crafting, approving and executing the budget. The role of the city council is mostly confined to amending limited parts of the budget bill crafted by the executive and, after spending has occurred, auditing and reviewing municipal spending. Moreover, given that in Brazil the budget law is not mandatory but just authoritative, the executive has large flexibility in deciding whether to execute or not each amendment approved by the city council. The annual budget follows the civil calendar and must be approved before a new year begins, *i.e.* the budget for year t is approved in year $t - 1$. This implies that mayors who are in their first term start their mandate with a budget crafted by the previous administration [Blöndal et al., 2003, Alston et al., 2005, Albuquerque et al., 2013].

The current constitution, enacted in 1988, promoted an important decentralization of the administrative structure, leading to an increase in the responsibility of city governments in the provision of public goods. The main areas under municipal responsibility are education (child care, primary and middle school), basic health services, provision of infrastructure in sanitation, transportation and urban planning.

Even though many expenditure categories have been decentralized to cities, tax collection continues to be rather centralized at the federal and state level. As a consequence, municipalities have relatively low self-financing capacity and are highly dependent on inter-governmental transfers, which accounted for 58 percent of all municipal revenues in 2016. Most of these revenues come from block-grant/earmarked transfer programs and a smaller share in the form of discretionary transfers.

Since the enactment of the Law of Fiscal Responsibility in 2000, municipalities (as well as other levels of government) face strong restrictions in their levels of deficit and debt.

3.2.2 Political parties and social cleavages

Brazil is a case of multipartism, with 33 registered and roughly 14 effective parliamentary parties in 2016 [Nicolau, 2017, Gallagher and Mitchell, 2005].⁵ Four parties, however, have played a major role in the period under study, both at the national and local level. The social-democratic, pro-Labor Worker's Party (PT) is dominant on the left and has won four consecutive presidential elections since 2002. The PT has moved towards the center during its bid to the 2002 presidential campaign [Campello, 2016]. The leftist camp also includes smaller communist, socialist and green parties. PSDB and MDB/PMDB are

⁵The effective number of parliamentary parties is a standard measure of political fragmentation in comparative politics, and is computed using the number of parties in parliament weighted by parties' vote shares [Laakso and Taagepera, 1979, Gallagher and Mitchell, 2005].

the main center-right parties, while DEM/PFL is the most important party on the right [Zucco and Power, 2009, Cesar Zucco and Power, 2021].⁶

Despite high fragmentation, the left-right divide is rather clear and highly relevant in Brazilian politics. For example, Cesar Zucco and Power [2021] find that the optimal number of clusters for classifying federal legislators along several ideological dimensions is two: a left-wing and a conservative camp. In other words, despite the large number of parties, a binary left-right classification is able to capture most ideological variation in Brazilian politics.

Using various measures of polarization, Cesar Zucco and Power [2021, p.18] also show that differences in policy preferences between left-wing and conservative legislators have been relevant and broadly stable or mildly increasing in our sample period (after a marked decrease in polarization between 1990 and 2002). Although Cesar Zucco and Power [2021] survey federal legislators, and therefore their results do not necessarily apply identically to local politicians, it is worth noting that around 37% of the national legislators in their sample is a former mayor or vice mayor.

There is also evidence that left-wing and conservative parties represent different constituencies and that, at least since the early 2000s, lower income Brazilians constitute the left's electoral base. In his recent historical comparative analysis of national political, ideological and economic regimes, Thomas Piketty summarizes and interprets the Brazil-

⁶Appendix C provides the full list of parties that participated in the municipal elections we study.

ian evidence as revealing “a *classist* party system emerging in the period 1989-2018 with important consequences for redistribution”[Piketty, 2020, p.953, our emphasis]. Specifically, Gethin and Morgan [2018] show that throughout all our sample period (2004-2016), Brazilian lower income classes were substantially more likely than economic elites to vote for left-wing parties in national elections. For example, in the 2014 presidential election, the poorest 50% (in terms of income) were more likely to vote left than the richest 10% by 23 percentage points. This pattern is visible at the regional as well as the individual level, with poorer regions (particularly the Northeast) increasingly voting left, and wealthier ones (in particular the South) increasingly leaning conservative [Zucco, 2008, Gethin and Morgan, 2018]. These class cleavages appear to be strongly linked to welfare policies directed to poor households that left parties (and the PT in particular) promoted at the national level [Gethin and Morgan, 2018, Zucco and Power, 2013, Zucco, 2008].⁷

3.3 Data

We combine electoral results from the 2004, 2008 and 2012 Brazilian municipal elections with data on several public finance outcomes. Our sample includes 8,943 municipal elections for which we can calculate the left’s margin of victory/loss (the running variable in our RDD) and have data on the fiscal policy outcomes of interest over the full post-election mayoral term.

⁷The electoral rise of the leftist PT in the poorer Northeast region has also been linked to an intentional investment in local organizational facilities in the region (including the network of local branches), especially in regard to local elections [Dyck, 2014].

3.3.1 Electoral results and partisanship

Data on municipal elections come from Brazil’s Electoral Court - *Tribunal Superior Eleitoral* (TSE). We focus on the 2004, 2008 and 2012 elections for two reasons. First, data for previous elections in the main TSE statistical repository is incomplete.⁸ Second, the fiscal outcome variables are not fully comparable in the pre-2002 period. Despite these challenges, in Appendix C we extend our sample to include the 1996 and 2000 elections by downloading electoral results from an old TSE repository and using an alternative definition of our outcome variables that allows consistency over time.⁹

From TSE, we obtain information on the candidate’s party, the composition of her coalition and the number of votes. With this information, we can compute the running variable in our RDD: the left’s margin of victory/loss, defined as the vote share of the most voted left-wing candidate minus the vote share of the most voted non-left candidate. In case of a runoff, we use the runoff vote shares to compute the margin of victory. We use the Cesar Zucco and Power [2021] classification to determine the ideological stance of parties (left or non-left). When a party is not included in Cesar Zucco and Power [2021], we use other sources to assign party ideology. The partisanship classification is detailed in Appendix C.

⁸This is clear from basic inspection of the data and is also stated in the TSE website (accessed on Sep 2020).

⁹The old TSE repository has all the key electoral variables for our study, but it is less precise than the main repository because it does not have detailed information about the status of the election or the candidates. See Appendix C for details. Both repositories were accessed in September 2020.

3.3.2 Public finance

Public finance data come from Brazil’s National Treasury - *Secretaria do Tesouro Nacional* (STN). Municipalities report detailed information on expenditures and revenues to STN, which then publishes the dataset *Finanças do Brasil - Dados Contábeis dos Municípios* (FINBRA).

We use total revenues and expenditures per capita and as a share of GDP as our measures of government size. Variables expressed in per-capita terms are measured in constant 2016 Reais using the GDP deflator.

We also study how the allocation of expenditures among the main budget and functional categories is affected by party ideology. For the budget categories, we use current expenditures, personnel and investment as a share of total expenditures. Given the main areas under responsibility of municipalities, we study the allocation of functional categories in two groups: social and non-social expenditures. We define social expenditures as expenditures in health and sanitation, education and culture, and social welfare programs. Other expenditures are composed of housing and urban development, transportation and others, the latter being a residual group that includes all other functional categories.¹⁰

¹⁰Pension expenditure is not part of social spending, and is included in the residual category. Unlike public pensions paid by national governments, that constitute a form of social protection directed to the population, pension spending by municipal governments just includes pensions paid to former municipal employees. It is therefore best interpreted as deferred personnel compensation, rather than a form of social spending. It should also be noted that pension expenditures are mostly pre-determined for the current mayor, as they reflect past hiring and wage-setting decisions by previous administrations. Appendix C shows that main results are qualitatively similar when including pensions paid to former employees into the social expenditure category.

To create a sample of oil-windfall receivers (used in mechanisms’ analysis), we use information from the *Transferências Constitucionais* from STN. This database reports all non-discretionary transfers made by the central government to states and municipalities.

Data on federal transfers received through congress amendments to the federal budget, which will be used in assessing our research design, come from *SIGA-Brasil*, a website of the Brazilian Senate containing detailed information on the federal budget.

3.3.3 Municipal characteristics

We supplement our data with municipal characteristics obtained from Brazil’s National Bureau of Statistics - *Instituto Brasileiro de Geografia e Estatística* (IBGE). Municipal GDP is from the publication *Produto Interno Bruto dos Municípios 2002-2016* [IBGE, 2010]. Population comes from the 2000 and 2010 Census and from the publication *Estimativas da População* [IBGE, 2018] in non-census years. All other demographic variables – median earnings, urbanization rate, race, labor force participation and education – come from the 2000 and 2010 Census. We also use information at the city level on the cash-transfer program Bolsa Família, obtained from *Ministério da Cidadania*.

3.3.4 Sample selection and descriptive statistics

We take a number of steps to create our baseline sample. We start with all 16,692 municipal electoral results available in the TSE repository. We exclude 256 elections which occurred after the regular schedule. After computing the left’s margin of victory/loss, the running variable in our RDD, we are left with 9,944 elections. Concerning the outcome

variables, even though FINBRA is an unbalanced panel dataset, it has a coverage rate of at least 93 percent of the municipalities per year. We only keep observations for which we can observe all fiscal policy variables over the full term. As a result, our baseline sample has 8,943 observations, where an observation is a municipality-election cycle.

Table 3.1 reports descriptive statistics for all our outcome variables, in our baseline sample and in the sub-samples we use to analyze mechanisms. Even though these sub-samples select observations following different criteria (discussed in detail in next sections), overall they are representative of our baseline sample. The same happens with all other city characteristics, except geographic location, as reported in Appendix Table C.2.

3.4 Research design

We employ a regression-discontinuity design (RDD) [Hahn et al., 2001] to identify the effect of a mayor’s partisanship on local fiscal policy. Intuitively, we estimate a causal effect by comparing municipalities with closely-elected left-wing mayors with municipalities where the left-wing candidate barely lost the election. More precisely, we test whether the expected values of our fiscal policy variables of interest display a discontinuity when the left margin crosses the victory threshold.

3.4.1 Regression-discontinuity specification

Our estimator of interest, which gives the local average effect of a left-wing mayor on fiscal policy variable y , is given by

$$\beta = \lim_{ml \downarrow 0} E[y|ml] - \lim_{ml \uparrow 0} E[y|ml] \quad (3.1)$$

where ml is the margin of victory/loss of the left candidate, defined as the difference between the vote share of the most-voted left-wing candidate and the vote share of the most-voted non-left candidate.

Our key identification assumption is that unobserved confounding factors – variables affecting both election probabilities and fiscal policy choices – do not ‘jump’ discontinuously around the threshold.¹¹ This means that cities where the left candidate barely wins an election do not tend to be very different from cities where the left barely loses. Under this ‘smoothness’ assumption, our RD estimator identifies the average causal impact of a (closely-elected) left mayor on fiscal policy variables.

We estimate β through the following RD specification:

$$y_{it} = \beta \mathbb{1}\{ml_{it} > 0\} + f(ml_{it}) + \alpha_i + \tau_t + \varepsilon_{it} \quad (3.2)$$

where i and t index city and election year; y is a public finance variable measured as an average over the after-election mayoral term, that is, from year $t + 1$ until year $t + 4$ (see Section 3.2); ml is the left’s margin of victory/loss; $f(\cdot)$ is a potentially non-linear function

¹¹More precisely, counterfactual outcomes are assumed to be continuous in the running variable.

that we approximate through kernel-weighted local linear regression; α_i and τ_t are city and year fixed effects.¹² We use the Calonico et al. [2014] robust and bias-corrected estimator.

3.4.2 Design assessment

To assess the validity of our RD design, we test for discontinuities in pre-determined covariates at the threshold. Table 3.2 displays differences in pre-determined city characteristics between cities with left and non-left mayors. The first column includes all elections, showing that in general cities electing a leftist mayor are different: they are much larger in terms of population, more likely to be urban, in the northeast region, and have a lower share of white population. These cities have also larger median earnings, but, at the same time, receive more conditional cash transfers (Bolsa Familia).

Given that Brazilian deputies tend to use federal budget amendments to reward cities that were important to their election, which in turn affects local voting behavior [Firpo et al., 2015], we also consider transfers received through these amendments as a potentially relevant covariate. We do not find any significant difference among cities with left and non-left mayors in this respect.

¹²We control for city and year fixed effects by first regressing y_{it} on city and year dummies, and then using residuals from this fixed-effects regression as the left-hand variable in our RD estimation. In the interest of efficiency, this first-step fixed-effects regression uses the whole sample, including observations which are excluded from the RD estimation because the ‘left margin’ variable is not available. Our results do not change if we restrict the first-step regression to exactly the same sample used in the RD estimation. See Lee and Lemieux [2010, p. 331-333] for details on this two-steps procedure, including the demonstration that no correction for the first step is needed when calculating standard errors.

Table 3.2 shows that any difference in pre-determined characteristics disappears if we restrict the comparison to progressively closer elections. Most importantly, column 5 estimates differences in pre-determined city characteristics using the same RD specification that we employ for estimating fiscal policy effects (equation 3.2), finding that all differences are both economically and statistically insignificant around the threshold.

Appendix Figure C.1 shows that there is no sign of systematic electoral manipulation in favor of or against left candidates: we do not find any discontinuity in the distribution of the running variable at the cutoff [McCrary, 2008]. Following Caughey and Sekhon [2011, p.392], in Appendix Figures C.2 and C.3 we test for a discontinuity in the vote share of the incumbent mayor or the incumbent party at the threshold, in order to test for possible electoral manipulation (or other forms of sorting) by incumbents, and find none.

In Appendix C, we also test for discontinuities in candidates' characteristics between bare winners and bare losers within both political camps, in the spirit of Caughey and Sekhon [2011]. We do not find evidence of sorting based on incumbency, education level or personal wealth. When not including city and time fixed effects in our specification, there is some discontinuity in campaign expenditures, with winning candidates spending more than losing candidates also at the threshold. However, when controlling for fixed effects as we do in our main analysis, these discontinuities in campaign expenditures shrink in size, becoming very small.¹³

¹³Evidence of discontinuities in campaign expenditures, among several other variables, has been found for a sample of US House elections by Caughey and Sekhon [2011]. To our knowledge, this a novel result in the context of Brazilian elections. While this type of discontinuity would pose obvious problems for

3.5 Main results: impact of partisanship on municipal fiscal policy

This section presents our main results, which are reported in the first column of Table 3.3 and displayed graphically in Figure 3.1. As explained in Section 3.4, all outcomes are measured as an average over the four years in office. Overall, we find no significant effect of left-wing mayors on the size of the city government nor on the allocation of expenditures across the main economic categories (current spending, investment and personnel). We do find a modest but precisely estimated positive effect on the social expenditures share.

3.5.1 Size of government

We proxy the size of city governments using their total revenues and expenditures, per capita and as a share of municipal GDP. We find no significant partisan effects on the size of city government: there is no discontinuity at the threshold for any of the four proxies (top panel of Table 3.3, column 1; Figure 3.1, panel (a)).

3.5.2 Budget composition

We now study how partisanship affects the allocation of municipal resources. First, we look at the composition of expenditures across the main budget categories. Again, we

analyses estimating incumbency advantage effects, it is much less clear how the ability of the candidate to raise funds would affect his fiscal policy choices once elected. In other words, ability to raise campaign funds is a natural covariate when the outcome is ability to win elections, but not when it is the allocation of the city budget. Moreover, besides becoming very small after controlling for fixed effects, these positive discontinuities in campaign expenditures are completely absent in the subsample of municipalities receiving oil windfalls, which are the ones where we find strongest fiscal policy effects. Furthermore, as argued in de la Cuesta and Imai [2016, p.384] and Eggers et al. [2015, pp.267-270] in reference to similar results for US House elections, this type of sorting would need to be implausibly precise in order to introduce significant bias.

find no significant effects: there is no evidence of discontinuities in the shares of current spending, personnel and investment in total spending (middle panel of Table 3.3, column 1; Figure 3.1, panel (b)). In Appendix C, we also look at the composition of revenues, finding no significant effect on the relative shares of municipal taxes, state transfers and federal transfers.

Second, we look at the composition of expenditures across the main functions of government. We find a modest positive discontinuity in the share of social expenditures (Figure 3.1, panel (b)). The share of social spending is higher by 0.6 percentage points under a left-wing mayor, with $p < 0.01$ (bottom panel of Table 3.3, first column).

All the main types of social expenditures display small average increases in their shares, with the ‘Education & Culture’ category increasing slightly more than ‘Health & Sanitation’ and ‘Social Welfare’. All other categories seem to adjust to accommodate the increase in social expenditures: we do not find any single item among non-social expenditures which tends to be disproportionately penalized. Indeed, when looking at sub-categories within other expenditures (housing, transportation, and all others), we find negative effects on all of them, but imprecisely estimated. This means that all other expenditures are on average reduced in relative size to make room for the relative increase in social spending, and there is large variation in how the ‘burden’ is distributed among other expenditure categories.¹⁴

¹⁴The ‘Social expenditures’ and ‘Other expenditures’ categories are exhaustive and mutually exclusive: they sum up to total expenditures. Therefore, the share of other expenditures decreases one-for-one with every increase in the social expenditures share. The effect on the other expenditures share and its standard error are thus necessarily of identical magnitude but opposite sign than those on the social expenditures

This reallocation translates into a small positive effect on the level of social spending per capita, which is larger by around 1 percent under a left-wing mayor (bottom panel of Table 3.3, first column).

In order to uncover dynamics, Appendix C reports results by year in office. The key finding is summarized in Figure 3.2: the effect on the social expenditures share increases gradually, reaching 1 percentage point in the last year in office, consistent with the idea that it takes time to reallocate resources. Effects on size of government and other budget categories are confirmed to be small and insignificant for all years in office (Appendix Table C.4).

To provide a broader view of how partisan effects have evolved over time after Brazil's democratization, Appendix C extends our sample period backwards to the 1996 and 2000 elections and plots the effect of a left-wing mayor on the share of social spending by electoral cycle.¹⁵ The effect on the share of social spending is positive in the entire time period, but appears much stronger in the 'boom years' 2004-2008. This appears consistent with the idea that left-wing mayors redistribute more when their financial constraints are relaxed, given that the 'boom years' were characterized by rising revenues due both to strong income growth and increasing oil royalties. We explore this idea in greater detail in the next section.

share – that is why we don't report them in our results tables, given that the information is already fully contained in the coefficient on the social expenditures share.

¹⁵It is important to note that the fiscal and electoral data is not fully comparable pre-2004. For details, see Appendix C.

3.5.3 Robustness and falsification tests

Robustness tests

Appendix C reports robustness tests. In the first column of Appendix Table C.7 we estimate effects on changes in fiscal outcomes rather than levels. In particular, for each outcome, we take the percentage-points difference between the election year ($t = -1$) and the fourth year of the term ($t = 4$). Our main results are qualitatively confirmed when using this approach.

In Appendix Table C.8 we exclude the first year of the mandate from the term average. This test is informative because mayors have limited influence on the budget in the first year of their mandate, which was written by the previous administration (as discussed in Section 3.2.1). Results regarding the size of government and budget categories remain very small and not significant, while the effect on the share of social spending, as expected, becomes slightly larger (almost 0.8 percentage points).

Appendix Table C.9 restricts the analysis to progressively larger cities, up to the 90th percentile. Results are overall rather stable, suggesting that heterogeneous effects by city size do not account for our results. Appendix Table C.10 re-estimates equation 3.2 using alternative bandwidth selection criteria. Results are insensitive to bandwidth choice.

Lagged outcomes

As a falsification test, Figure 3.2 and the first column of Appendix Table C.4 report results for the ‘effect’ of a left-wing mayor on lagged (pre-election) outcomes. Reassuringly, we find no significant effects on lagged outcomes.

Placebo thresholds

To assess whether our RD specification tends to over-reject the null hypothesis or suffers from some other failure of its identification assumptions, we perform a falsification test using placebo thresholds. We randomly draw 200 placebo thresholds, a hundred from each side of the true threshold, and re-estimate equation 3.2 with social expenditures (as a share of spending or per capita) as the outcome variable and using a placebo threshold instead of the true threshold. In order to avoid mis-specification, we only include in the estimations observations from the same side of the true threshold. We consider only placebo thresholds that guarantee at least 25 observations in each side of the bandwidth to avoid biasing the test against significant findings due to weak statistical power.

Figure 3.3 plots the distribution of t-statistics from the regressions using randomly-drawn placebo thresholds. There is no evidence of a tendency to find significant discontinuities away from the true threshold. Moreover, the t-statistics from our baseline estimations at the true threshold (vertical dashed lines) are in the tails of the distribution of placebo t-statistics and are consistent with levels of significance below 1 percent for the share of social expenditures and below 5 percent for social expenditures per capita.

3.6 Mechanisms: what accounts for substantial fiscal policy convergence in Brazilian cities?

This section explores potential explanations for the rather limited extent of the partisan effects we have found. Our analysis below suggests that policy divergence in the allocation

of spending is limited by both institutional constraints and re-election concerns. In contrast with previous evidence for the US, Tiebout competition does not seem to play a significant role in our sample. Moreover, we do not find any evidence that our results are driven by ideological heterogeneity within mayoral coalitions.

3.6.1 Re-election concerns

Political competition is a natural candidate explanation for policy convergence, in the spirit of the classic Downsian model. We test this explanation by restricting the analysis to ‘lame-duck’ mayors, who cannot run for re-election because of term-limits. Of course, this does not eliminate political competition effects altogether – ‘lame-duck’ mayors may still care about their party/coalition winning the next election, or about their own reputation, for example in view of running for other offices – but it can weaken them significantly.¹⁶

Results are consistent with re-election concerns playing a role in limiting budget composition effects (second column of Table 3.3 and middle panel of Figure 3.5). In the sub-sample with ‘lame-duck’ mayors, partisan effects on the share of social expenditures are indeed moderately larger than in the whole sample (1.3 versus 0.6 percentage points). Furthermore, in terms of social expenditures per capita, effects are almost three times larger in this subsample (3.3 versus only 1 percent in the baseline sample). In contrast,

¹⁶This test follows a large previous literature that focuses on policy makers facing binding term limits in order to study the effect of weakening electoral competition pressures (e.g., Besley and Case [1995b], Ferraz and Finan [2011]). Previous studies suggest that ‘lame-duck effects’ are present among Brazilian mayors. In particular, Ferraz and Finan [2011] find that Brazilian lame-duck mayors are significantly more likely to engage in corruption than those facing re-election incentives.

the effect on the size of the city government remains non-significant, suggesting that for this variable absence of partisan effects is not driven by re-election concerns.

3.6.2 Tiebout competition

Competition between cities within a geographical area (‘Tiebout competition’) may limit the policy space of mayors if residents can easily move to nearby cities [Tiebout, 1956, Peterson, 1981]. Ferreira and Gyourko [2009] find evidence that this mechanism can explain policy convergence between Democrat and Republican mayors in US cities.

To test this hypothesis, we follow Ferreira and Gyourko [2009] in building a proxy for the intensity of Tiebout competition faced by each city in our sample. This measure of Tiebout competition is a Herfindahl index of the adult population (at least 16 years old) in each city within a commuting zone (*microregião*). This is calculated as the sum of the squares of the shares of population of the municipalities inside the same commuting zone [Ferreira and Gyourko, 2009, 417]. A low value for this index indicates high Tiebout competition: many cities of small relative size within the same commuting zone; symmetrically, a high value signals low competition.

To assess whether Tiebout competition can explain our baseline results, we restrict the analysis to cities facing low Tiebout competition. Under the hypothesis that Tiebout competition explains policy convergence, we expect larger partisan effects in these cities. The third column table 3.3 reports our RD specification in cities with below-median Tiebout competition (Herfindahl index above the median). In Appendix Table C.11, we restrict the

sample to cities facing even lower competition, with Tiebout competition below the 25th percentile (Herfindahl index above the 75th percentile).

Overall, we do not find support for the Tiebout competition hypothesis. Effects on the size of government and on distribution among functional categories remain insignificant. Effects on the share of social expenditures get moderately larger in the sample with below-median Tiebout competition, but smaller in the sample with below-25th-percentile competition. Moreover, we find little effect on social expenditures per capita in both subsamples. These results are inconsistent with the Tiebout-competition hypothesis, which predicts that partisan effects should grow in size as the intensity of Tiebout competition gets lower.

3.6.3 Institutional constraints

As discussed in Section 3.2.1, Brazilian mayors appear to face strong institutional constraints on their fiscal policy decisions: laws regulating local public finance, limited tax capacity, and hardwired expenditures. If binding, these constraints may help explain limited policy divergence.

To test this hypothesis, we look at cities and periods in which institutional constraints are exogenously relaxed by ‘oil-windfalls’: large increases in revenues due to royalties from oil production.¹⁷ If policy divergence is limited by institutional constraints, we expect

¹⁷Caselli and Michaels [2013, pp. 117-221 and online appendix] provide details on the rules governing the allocation of oil royalties in Brazil. Note that our RD strategy does not rely on the assumption that oil windfalls are exogenous to city-level characteristics and local shocks: we do not compare outcomes for cities receiving oil windfalls versus other cities. Our comparison is between closely elected left and non-left

to find larger effects (larger policy divergence) when these constraints are relaxed by oil windfalls.

Large oil windfalls are relatively common in our sample. Since the 1997 ‘New Oil Law’, companies must pay royalties amounting to 5 to 10 percent of oil output value, indexed at international prices. Most of these royalties are allocated to local governments, following rules that disproportionately benefit a set of “oil producing” municipalities. Moreover, both oil production and oil prices displayed large increases in the period we study, resulting in sudden substantial relaxations of the budget constraints of ‘oil producing’ cities.

Importantly, cities are relatively free in allocating revenues from oil royalties, with only two restrictions: these revenues cannot be directly used to hire new public employees on a permanent basis, nor to pay public debt. Caselli and Michaels [2013] show that on average (independent of partisanship) oil revenues tend to increase municipal spending on housing, urban development, transportation, education, health and transfers to households, but have little effect on welfare-related outcomes, with the exception of the education sector.

To identify a subsample of oil windfall receivers, we use the growth of average oil royalties received over the mayoral term.¹⁸ We calculate this variable for each ‘oil-producing’ municipality in our sample and define our oil-windfall receivers subsample as those observations above the median. In other words, a city-election enters our ‘oil-windfall’ subsample

mayors, within the subset of cities which experienced an increase in oil windfalls. What we do assume here is that oil production and oil prices are not affected by a mayor’s fiscal policy choices and that heterogeneity in partisan effects between cities receiving oil windfalls and other cities are due to oil windfalls and not to other differences.

¹⁸To properly take into account city size, we scale the change in oil royalties by previous-period revenues.

if it experiences a relatively large (above-median) change in revenues from oil royalties during the (after-election) mayoral term. In our baseline sample, 919 observations satisfy this criterion.

Partisan effects on the budget composition are indeed much stronger in the presence of oil windfalls (last column of Table 3.3 and panel (c) of Figure 3.5). The election of a left-wing mayor raises the share of social expenditures by 2.2 percentage points in this setting – a more than threefold increase in the size of the effect relative to the baseline. The differential effect is even larger in terms of social expenditures per capita: 6.5 versus only 1 percent in the baseline.

The effect on the overall size of the city government, as measured by municipal revenues and expenditures over GDP, however, remains essentially null as in all other specifications and subsamples (column 5 of Table 3.3). This suggests that the absence of partisan effects on government size is not driven by the strong institutional constraints faced by Brazilian mayors, but rather by absence of underlying ideological differences between Brazilian mainstream parties on this topic, although we cannot rule out alternative explanations based on external constraints different from the ones we have been able to identify and measure.

3.6.4 Ideological convergence between mayoral coalitions

Another possible explanation for policy convergence is high ideological heterogeneity within political coalitions supporting mayors.¹⁹ In some elections, conservative parties are part of the coalition supporting a left mayor, and vice versa. Heterogeneity within each coalition could lead to smaller ideological differences in policy platforms between different coalitions, therefore reducing policy divergence. Moreover, this channel could be stronger in close elections: when an election is expected to be tight, there might be a stronger incentive for left and conservative candidates to try to secure the support of some party from the opposite ideological camp, in an attempt to maximize electoral returns.

To test this hypothesis, we calculate a measure of ideological distance between the competing coalitions in each election, and then assess how results vary with this index. If the small extent of policy divergence in our baseline results is due to a pre-electoral coalition-making process that leads to ideologically ambiguous coalitions, we would expect results to be different in elections in which the ideological distance between competing coalitions is large. We first compute a left-right ideology score for each mayoral coalition in our sample. This is equal to the weighted average of the Cesar Zucco and Power [2021] left-right score for all parties in the coalition, with weights given by parties' vote shares in the

¹⁹In Brazilian mayoral elections, coalition building happens before the election, presumably based on expected electoral returns, and aims to achieve mostly two things: i) get potential opponents out of the way and ii) transfer of electoral resources, especially fractions of free campaigning time on TV and radio [Limongi and Vassalai, 2018].

previous city council election.²⁰ We then measure ideological distance as the difference in this left-right ideology score between the coalition supporting the most-voted left candidate and the coalition supporting the most-voted conservative candidate.

A first relevant fact is that, at least on average, there is a clear ideological demarcation between the competing coalitions that we study. In our average election, the coalition supporting the most-voted left-wing candidate is to the left of the coalition supporting the most-voted conservative candidate by around 0.37 points (median = 0.45), in a ideological score that ranges from -1 to +1. There is, however, substantial variability: the standard deviation for this distance is 0.42.

Moreover, coalitions supporting left mayors are clearly to the left of those supporting conservative mayors in the close elections we use for identification. To see this, we run our baseline RD specification, using the left-right ideology score for the elected mayor's coalition as the outcome variable and the left margin as the running variable. At the threshold, the ideology score for the winning coalition jumps to the left by 0.36 points (s.e. 0.02). This is shown in Figure 3.4. This is clearly inconsistent with the hypothesis that in close elections the competing coalitions tend to be ideologically indistinguishable.

Results reported in Table 3.3 and Appendix Table C.11 also provide little support for an explanation of our results based on ideological heterogeneity within coalitions. When

²⁰Similar to our approach, Fujiwara [2015] and Power and Rodrigues-silveira [2019] also use weighted-averages of the Cesar Zucco and Power [2021] left-right scores to measure, respectively, the ideological position of state legislatures and the electorally expressed ideology of Brazilian voters at the municipal level.

restricting the analysis to elections in which the ideological distance between coalitions is larger (above median and above the 75th percentile), results remain qualitatively similar to the baseline: there is no significant effect of left-wing mayors on the size of government; the effect on the social expenditures share remains small (only slightly larger than in the baseline) and actually decreases when passing from the median to the 75th percentile of ideological distance.

3.6.5 Inference on differential impacts

We perform a simple bootstrap exercise to provide more information on the differential impacts presented in our mechanisms' analysis. For each mechanism, we estimate our RD specification separately in the subsample of interest and in the rest of the sample. We then take the difference between the two estimated effects and run 500 bootstrap replications to obtain standard errors (clustered by municipality) for this difference.²¹

As shown in Table 3.4, the differential effects on social spending in cities with lame duck mayors and in those receiving oil-windfalls, both measured as a share of expenditures and per capita, are all significant at least at the 10% significance level. Instead, effects in the subsamples with low Tiebout competition or high ideological distance between coalitions are not statistically different from the rest of the sample.

²¹This procedure is equivalent to using interaction terms to obtain differences in group effects in a parametric regression. We perform a bootstrap exercise due to our non-parametric approach. Point estimates for differential effects from this exercise reported in Table 3.4 are not equal to differences between coefficients from different columns of Table 3.3, because here we take the difference between the subsample and the rest of the observations, rather than between the subsample and the whole sample.

3.6.6 Dynamics, robustness and placebo tests for the mechanisms’ analysis

Figure 3.2 and Appendix C report dynamic effects, placebo exercises and robustness tests for our mechanisms’ analysis.

Like baseline ones, results are qualitatively robust to differencing fiscal outcomes (Appendix Table C.7), excluding the first year of the term (Appendix Table C.8) and to alternative bandwidth selection criteria (Appendix Table C.10). When using differenced outcomes, however, the precision of the mechanisms’ analysis decreases substantially (standard errors get wider). This is not surprising: in this robustness test we are considering only the final year of the term, which is likely to be noisier than the term-average, and at the same time the mechanisms’ analysis employs smaller samples.

Also the results of the mechanisms’ analysis pass the falsification exercise using placebo thresholds (Appendix Figure C.6). Moreover, there are no ‘effects’ on lagged (pre-election) outcomes (Appendix Tables C.5 and C.6). Social spending effects exhibit no pre-existing trend and increase gradually over time also in the ‘lame duck’ and ‘oil-windfall’ subsamples; they are larger compared to the baseline for each single year in office (Figure 3.2).

3.6.7 Welfare-related outcomes

In Appendix C, we estimate the effect of mayors’ partisanship on a limited number of welfare-related outcomes. Specifically, we look at measures of infrastructure, human resources and overall performance of the municipal educational and health care systems.

We also look at homicide rates given the high levels of violence observed in Brazil [Cerqueira et al., 2020].

This exercise should be interpreted as exploratory and taken with a grain of salt. First, the outcomes we use are very persistent and respond to policy with substantial time lags. Therefore, our research design might not be best suited to study them. Second, because of data availability, we are able to include a very limited number of outcomes, which provides only a partial and incomplete view of the possible socioeconomic effects of municipal policy.²² Third, given the small size of the effect on each of the components of social spending (third panel of Table 3.3), any effect on individual welfare-related outcomes would plausibly be very small, and therefore hard to distinguish from noise in a finite sample.

With these important caveats in mind, Appendix Table C.13 does not find any robust effect on these outcomes over the mayoral term. Overall, there is some sign of positive effects of left-wing mayors on some education outcomes – in particular reductions in average class size and student-to-teachers ratios, and increases in progression rates in municipal schools – but these estimates are very imprecise and not statistically significant. The estimated effect on the infant mortality rate has a negative sign, but is again very imprecisely estimated and far from statistically significant. Effects on number of doctors, number of clinics and homicide rates have varying signs and are imprecisely estimated. For the rea-

²²Our research design based on close elections requires annual data available for all years of each election cycle or, at least, available in a regular schedule with information available for all cycles (for example, data on the third year of mayoral terms). Moreover, we need data that are representative at the city level. In Brazil, most surveys do not fit these two criteria, limiting the inclusion of a broader range of outcomes in this exercise.

sons outlined above, we see these results as preliminary and not conclusive, and calling for further research.

3.7 Discussion

The most plausible explanation for our findings is that Brazilian left-wing mayors aim to redistribute municipal resources towards social spending in order to benefit the lower-income voters who constitute their electoral base, consistent with the available evidence on social cleavages in Brazilian politics (reviewed in Section 3.2.2).²³ Their ability to do so, however, appears to be severely limited both by institutional constraints and re-election concerns.

Viewed in this light, our results uncover some parallel between national and local political tendencies in Brazil. It is indeed widely recognized that, in the period we study, increasing social welfare spending has been a defining characteristic of the left-wing tenures in power at the federal level, and that financial constraints (mostly related to the global economic context) have determined the timing and generosity of welfare expansions [Campello, 2016].

Our results and the interpretation we have proposed are also consistent with Fujiwara [2015]’s analysis of the effects of electronic voting in Brazilian elections for state legislatures. Fujiwara [2015] finds that the introduction of electronic voting in the 1998-2002 period

²³Of course, given that our results are based on a comparison between (closely-elected) left-wing and conservative mayors, this is equivalent to saying that conservative mayors shift resources away from social spending and towards their preferred uses.

constituted a *de facto* enfranchisement of mainly poorer citizens. This caused the election of more left-wing state legislators, leading to a gradual increase in the share of social spending (in particular on public health care) but no effect on the size of the state government.

Although we find quite limited overall partisan effects on municipal fiscal policy, our mechanisms analysis does not support the widespread view that political parties in newly democratized developing countries lack distinctive economic policy preferences and programmatic linkages to voters. According to this view, parties in young democracies cannot rely on an already established social base, which would require both long time and substantial effort to develop. Therefore, they seek to win elections (especially local ones) by relying on clientelistic and charismatic appeals and self-interested local brokers. This strategy, in turn, inhibits the development of distinctive policy platforms and programmatic linkages to voters [Kitschelt and Wilkinson, 2007, Novaes, 2018]. This view would predict complete absence of partisan effects, including when institutional constraints and re-election concerns are relaxed, contrary to our analysis of the mechanisms that limit policy divergence.

Of course, our results are not in contradiction with the view that local brokers are important in local politics, especially in developing countries, and that their strategic rent-seeking behavior can weaken political parties at the local level (as documented for example in Novaes [2018] for Brazil and Camp [2017] for Argentina) and possibly also dilute their ideological identity. In fact, this might well be one of the mechanisms reducing policy divergence in our sample. However, our results are inconsistent with the view that

Brazilian parties are completely free of distinctive policy preferences and programmatic linkages to voters as a result of this type of mechanism.

Another relevant question is why Tiebout competition within commuting zones does not seem to play a major role in limiting fiscal policy divergence in our sample (unlike, for example, in the case of US cities, as found by Ferreira and Gyourko, 2009). Our data does not allow to provide a conclusive empirical answer to this question, but some considerations are possible. Municipal public service provision and municipal taxation might be of second order importance for location choices, when compared to other factors such as living costs, earnings and employment opportunities.²⁴ For this reason, the degree of policy divergence that would be required to significantly affect location dynamics might be larger than what other (institutional and political) constraints can plausibly allow, thus making Tiebout competition a non-binding constraint for Brazilian mayors.

In principle, high moving costs might also contribute to make Tiebout competition toothless. However, even if we cannot directly observe these moving costs, internal migration in Brazil appears rather intense [de Lima Amaral, 2013].²⁵

²⁴Indeed, the literature on internal migration in Brazil has generally identified real wage differentials and employment opportunities (together with geographical distance) as the key determinants of population movements [de Lima Amaral, 2013, Lameira et al., 2015]. Two important caveats are that existing studies (a) do not focus specifically on movements within commuting zones and (b) do not explicitly compare the importance of these factors to that of municipal public service provision.

²⁵An alternative explanation might be that Tiebout competition occurs across commuting zones as easily as within them, in such a way that the availability of alternative locations within the same commuting zone (which is what our measure, following the literature, captures) is not a relevant factor. For this to occur, however, moving within commuting zones would have to be at least as costly as moving across them. This appears unlikely, especially when taking not only monetary but also social costs into account, and inconsistent with available evidence. For example, Egger [2019] finds that Brazilian internal migrants tend to prefer towns closer to their origin, to minimize monetary and social moving costs.

Finally, one might ask whether political alignment with the federal government could contribute to explain our results. There is evidence that municipalities with mayors affiliated with the coalition in power at the federal level receive more discretionary transfers (Brollo and Nannicini 2012; Meireles 2019, Chapter 2).²⁶ This might be relevant, especially given that during our sample period the leftist PT held the Presidency of the federal government, heading a center-left government coalition (with the only exception of the last three months of the 2016 mayoral term).

However, this mechanism can hardly explain our results. First, if political alignment with the federal government was driving our results, it would have caused a positive effect of left mayors on the size of government and, possibly, investment. However, we find little effect of left mayors on revenues and expenditures, both per capita and as a share of GDP. Indeed, when analyzing the composition of revenues, we find no significant effect of left-wing mayors on federal transfers received (Appendix C). A possible reason is that conservative parties have been part of the center-left coalition in power at the federal level during our sample period and held ministerial positions (including the vice presidency). Therefore, some conservative mayors in our sample might have benefited from the discretionary transfer effects documented in Brollo and Nannicini [2012] and Meire-

²⁶Brollo and Nannicini [2012] find that municipalities with mayors affiliated to the coalition (and especially the political party) of the Brazilian President receive more discretionary transfers for infrastructure projects in preelection years. Meireles [2019, Chapter 2] shows that cities aligned with a ministry, meaning that the mayor and the minister are from the same party, receive on average 25% more voluntary transfers from that specific ministry.

les [2019]. Moreover, alignment with the federal government cannot explain the substantial differential effects found in our mechanisms analysis.

3.8 Conclusions

To study the role of partisanship in shaping local fiscal policy, this paper analyses a large sample of Brazilian municipal administrations in the 2004-2016 period. We employ a regression-discontinuity design, focusing on close mayoral elections.

We find no effect of left-wing mayors on the size of the city government, but a modest positive effect on the share of social expenditures. A left-wing mayor tends to raise the share of social expenditures by around 0.6 percentage points relative to a non-left mayor in our preferred RD specification. This reallocation results in a 1 percent increase in social spending per capita.

We then explore four potential mechanisms that may account for the lack of more substantial partisan effects. Our results suggest that re-election concerns and institutional constraints play a role in explaining the limited extent of budget composition effects. In cities that have their budget constraint relaxed by an ‘oil windfall’, the positive impact of a left-wing mayor on the share of social expenditures is more than three times larger than in the whole sample (around 2.2 percentage points). Also in the subsample of cities with ‘lame-duck’ mayors we find a larger effect on the share of social spending (1.3 percentage points compared to 0.60 in the baseline sample). These differential effects are even larger in terms of social expenditures per capita: on this variable we find a partisan effect of

almost 6.5 percent among oil-windfall receivers, and around 3.3 percent among ‘lame duck’ mayors, compared to just 1 percent in the whole sample. We find no empirical support for explanations based on Tiebout-competition or ideologically ambiguous electoral coalitions.

Our results, combined with available evidence on political cleavages in Brazil, suggest that Brazilian parties do attempt to shape the allocation of municipal resources to favor their respective constituencies, but their ability to do so is severely limited by institutional and budget constraints and re-election concerns. Our analysis of the factors limiting fiscal policy divergence in Brazilian cities suggests that, contrary to a widespread view, local politics in newly democratized developing countries can be characterized by distinct underlying policy preferences and programmatic linkages between voters and parties. Further studies might help determine whether Brazil is an exception or the rule from this point of view.

FIGURES AND TABLES

Table 3.1: Descriptive Statistics

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Size of government: overall revenues and expenses					
Expenditure per capita	781.50 (38.71)	783.86 (39.72)	784.30 (39.58)	779.11 (38.22)	778.17 (42.83)
Expenditure, % GDP	19.31 (13.12)	19.94 (17.02)	16.82 (13.77)	18.48 (10.88)	19.16 (10.16)
Revenue per capita	791.85 (40.09)	794.16 (41.19)	794.35 (40.67)	789.22 (39.88)	787.04 (42.42)
Revenue, % GDP	21.36 (13.89)	22.07 (18.08)	18.57 (14.68)	20.43 (12.03)	20.98 (11.24)
Allocation of resources: budget categories (% of total expenditure)					
Current Expenditure	88.20 (4.89)	87.44 (5.17)	88.01 (5.00)	88.40 (4.79)	88.56 (5.47)
of which:					
Personnel	48.12 (7.16)	47.54 (7.15)	48.10 (6.96)	48.57 (7.07)	48.25 (7.19)
Public Investments	10.20 (4.85)	10.94 (5.14)	10.38 (4.95)	9.97 (4.68)	9.58 (5.56)
Allocation of resources: functional categories (% of total expenditure)					
Social Expenditures	59.56 (8.49)	59.24 (8.57)	59.25 (7.99)	59.88 (8.67)	59.66 (8.06)
of which:					
Health & sanitation	24.14 (5.32)	23.91 (5.17)	24.36 (5.58)	24.26 (5.62)	22.54 (5.03)
Education & culture	31.64 (8.32)	31.52 (8.31)	31.20 (8.07)	31.88 (8.39)	33.57 (7.58)
Social welfare	3.79 (1.69)	3.81 (1.73)	3.69 (1.63)	3.74 (1.67)	3.55 (1.75)
Other Expenditures:					
Housing	8.98 (4.79)	9.43 (4.94)	9.55 (5.06)	8.86 (4.84)	10.87 (5.11)
Transportation	3.55 (4.33)	3.44 (4.26)	3.20 (3.92)	3.28 (4.19)	1.44 (2.23)
Other	27.90 (7.51)	27.90 (7.77)	28.01 (7.34)	27.98 (7.86)	28.03 (8.01)
Left candidate margin	-10.05 (27.67)	-11.57 (31.13)	-9.43 (28.57)	-7.06 (24.29)	-12.13 (31.82)
Observations	8943	2395	4158	3105	919

Notes: This table reports mean and standard deviation (in parenthesis) for the outcome variables and the left candidate margin of victory. Outcome variables are from FINBRA-STN and the margin of victory computed from the TSE electoral results. See Section 3.6 for the specific definition and motivation of each subsample. Summary statistics for covariates are in Appendix Table C.2.

Table 3.2: Difference in municipality characteristics between left and non-left mayors, by left margin of victory

	All	+/- 40	+/- 10	+/- 5	baseline RD
Labor market and demographic covariates					
log(Median earnings) $\times 100$	2.06 (0.64)	-0.41 (0.74)	0.44 (1.06)	-0.08 (1.43)	0.53 (0.54)
Labor force participation	-0.16 (0.18)	-0.03 (0.21)	0.15 (0.31)	0.12 (0.42)	0.25 (0.18)
log(Population) $\times 100$	27.90 (2.60)	3.57 (2.72)	3.45 (3.69)	-0.36 (4.90)	-0.04 (0.47)
% Urban	1.38 (0.51)	-0.30 (0.57)	-0.30 (0.79)	-0.83 (1.07)	-0.31 (0.23)
% White	-2.75 (0.52)	-1.78 (0.60)	-1.28 (0.88)	-1.62 (1.20)	-0.42 (0.23)
% Higher education	0.33 (0.07)	0.02 (0.07)	-0.01 (0.10)	-0.03 (0.14)	-0.02 (0.05)
% Illiterate	-0.00 (0.23)	0.33 (0.26)	0.01 (0.38)	0.17 (0.53)	-0.02 (0.11)
Geographic indicators					
North	-0.00 (0.01)	-0.01 (0.01)	-0.01 (0.01)	0.01 (0.01)	0.02 (0.02)
Northeast	0.04 (0.01)	0.02 (0.01)	0.01 (0.02)	0.01 (0.02)	-0.03 (0.04)
South	-0.01 (0.01)	-0.01 (0.01)	-0.00 (0.01)	0.01 (0.02)	0.02 (0.03)
Southeast	-0.00 (0.01)	0.01 (0.01)	-0.00 (0.01)	-0.03 (0.02)	-0.03 (0.03)
Midwest	-0.03 (0.01)	-0.01 (0.01)	0.00 (0.01)	0.01 (0.01)	0.01 (0.02)
Other covariates					
log(Bolsa Familia households) $\times 100$	9.56 (1.81)	7.83 (2.21)	1.26 (3.25)	2.07 (4.44)	-0.35 (1.90)
log(Bolsa Familia receipts) $\times 100$	10.82 (1.96)	8.32 (2.36)	1.77 (3.46)	2.76 (4.72)	-0.59 (1.97)
Authorized amendments	0.17 (0.12)	-0.11 (0.16)	-0.21 (0.16)	-0.04 (0.17)	-0.15 (0.24)
Executed amendments	0.04 (0.07)	-0.14 (0.13)	-0.15 (0.10)	-0.03 (0.07)	-0.01 (0.15)
Observations (all)	16427	7849	3400	1809	8943
Observations (effective)	16427	7849	3400	1809	4608

Notes: Standard errors clustered by municipality. Both the number of households receiving Bolsa Familia and Bolsa Familia receipts are normalized by population to take into account city size. Transfers received through amendments are expressed as a share of city revenues. Column 6 employs our baseline RD specification (equation 3.2), using the bias-corrected procedure of Calonico et al. [2014] and controlling for city-year fixed effects.

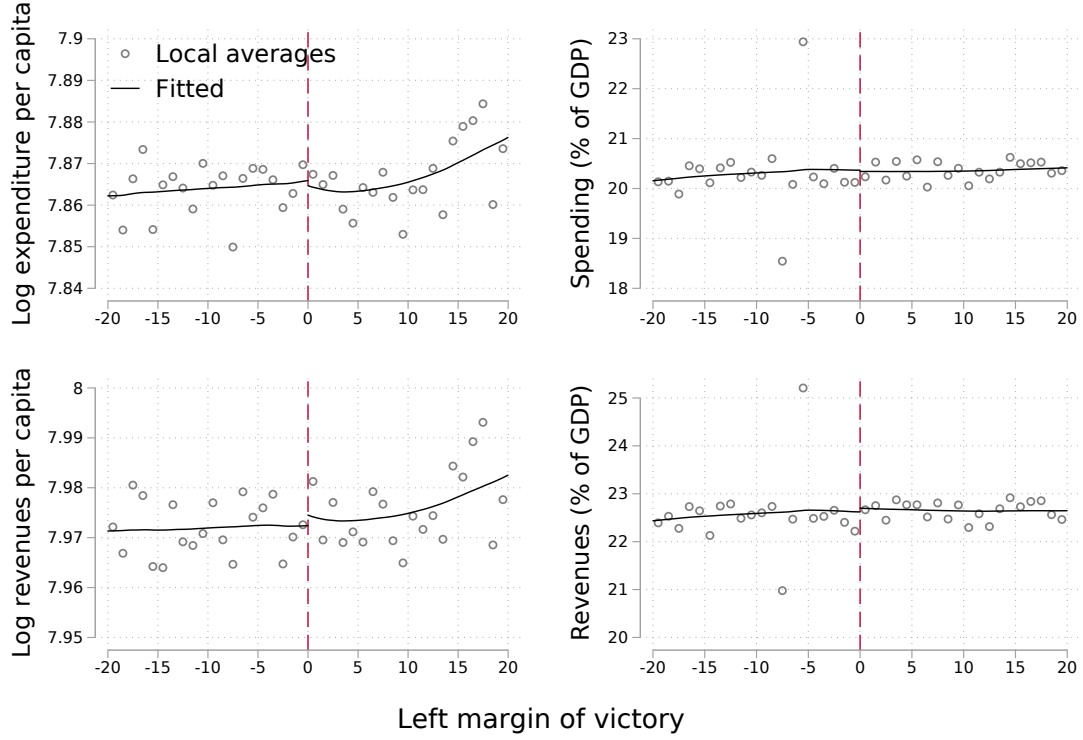
Table 3.3: RD estimates of the effect of a left-wing mayor

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Size of city government					
Expenditure per capita	-0.01 (0.56)	0.52 (1.27)	-1.24 (0.85)	0.23 (0.82)	2.26 (2.06)
Expenditure, % of GDP	0.01 (0.24)	-1.05 (1.18)	-0.36 (0.43)	-0.02 (0.25)	-0.13 (0.47)
Revenue per capita	0.39 (0.52)	0.59 (1.21)	-0.77 (0.77)	1.02 (0.88)	2.19 (2.01)
Revenue, % of GDP	0.12 (0.25)	-1.14 (1.20)	-0.20 (0.42)	0.24 (0.27)	-0.21 (0.49)
Allocation of resources: budget categories (% of total expenditure)					
Current Expenditure	-0.05 (0.17)	-0.39 (0.38)	-0.15 (0.25)	0.18 (0.31)	1.01 (0.65)
of which:					
Personnel	-0.05 (0.22)	-0.81 (0.48)	-0.04 (0.30)	-0.08 (0.41)	-0.34 (0.85)
Public Investment	0.09 (0.16)	0.40 (0.37)	0.23 (0.26)	-0.08 (0.31)	-0.96 (0.63)
Allocation of resources: functional categories (% of total expenditure)					
Social Expenditures	0.64 (0.21)	1.27 (0.40)	0.71 (0.30)	0.91 (0.39)	2.19 (0.87)
of which:					
Health & sanitation	0.18 (0.15)	0.69 (0.30)	0.38 (0.22)	-0.11 (0.27)	0.45 (0.42)
Education & culture	0.24 (0.16)	0.31 (0.30)	0.12 (0.19)	1.00 (0.30)	0.83 (0.50)
Social welfare	0.16 (0.06)	0.32 (0.11)	0.27 (0.09)	0.12 (0.10)	0.36 (0.20)
Other Expenditures:					
Housing	-0.16 (0.13)	-0.36 (0.29)	0.05 (0.21)	-0.15 (0.24)	0.41 (0.65)
Transportation	-0.18 (0.09)	-0.07 (0.20)	-0.24 (0.13)	-0.52 (0.19)	-0.73 (0.29)
Other	-0.21 (0.23)	-0.86 (0.42)	-0.59 (0.34)	-0.22 (0.40)	-1.90 (1.10)
Social Expenditures per capita	1.16 (0.61)	3.34 (1.36)	0.25 (0.86)	1.92 (0.93)	6.48 (2.40)
Observations (all)	8943	2395	4158	3105	919
Observations (effective)	4408	1227	2367	1660	451

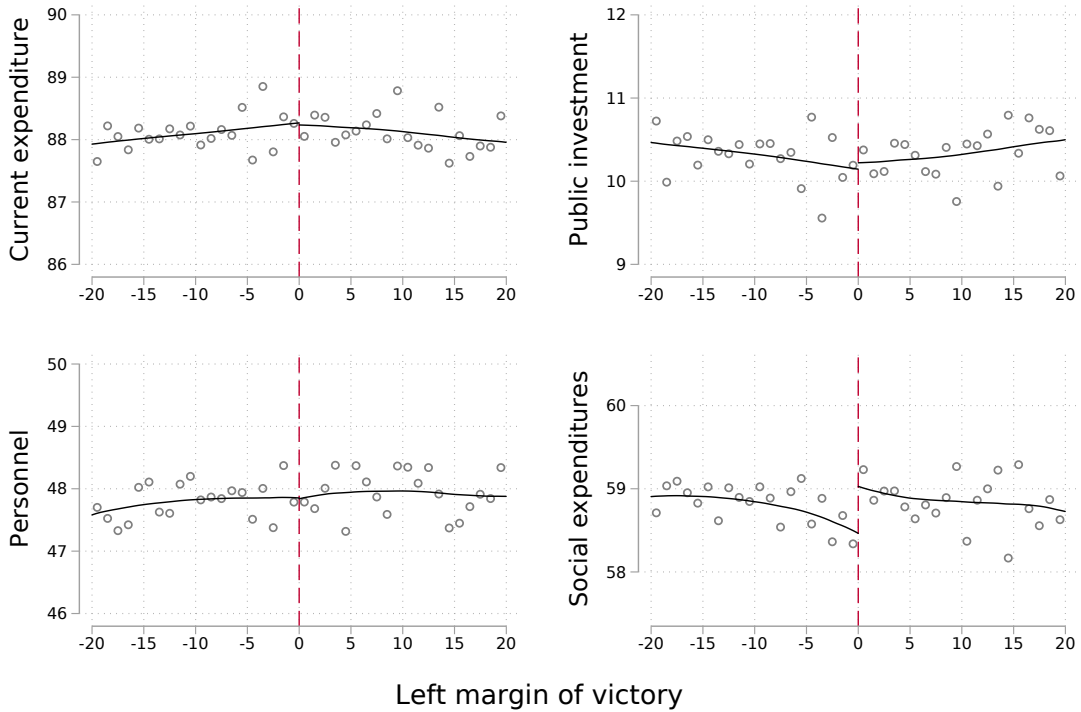
Notes: Estimation of equation 3.2, using the Calonico et al. [2014] procedure and controlling for city and year fixed effects. Outcomes are 4-year averages over a mayoral term. Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Figure 3.1: Local fiscal policy indicators - baseline (whole sample)

(a) *Size of city government*

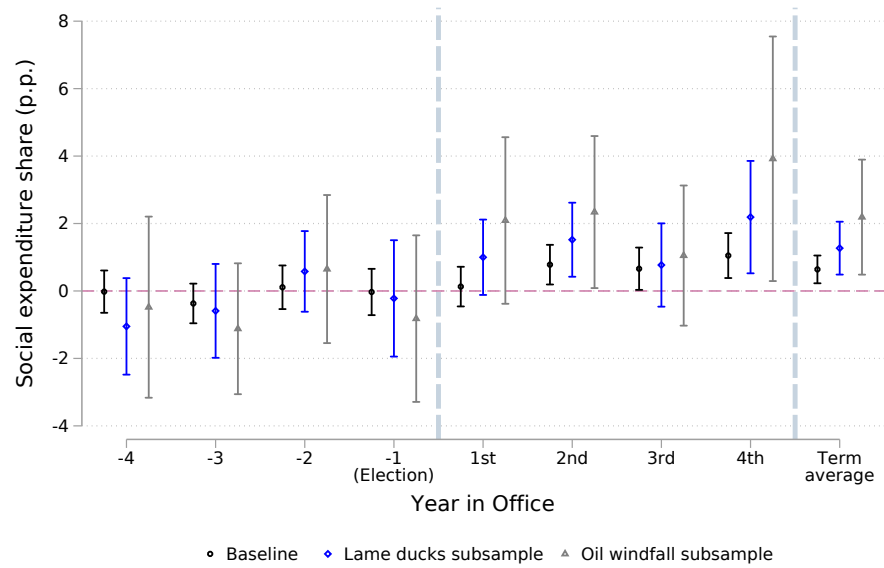


(b) *Expenditures composition (shares)*



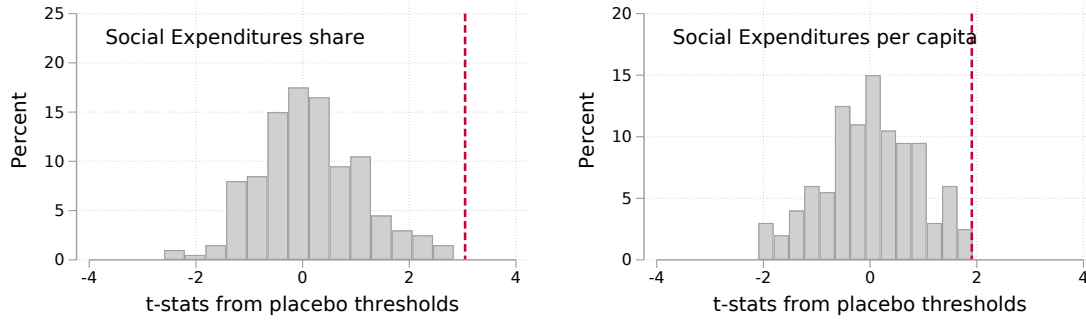
Notes: Visual presentation of our RD estimates of the effect of a left-wing mayor, reported in column 1 of table 3.3 and based on the specification in equation 3.2. All outcomes are 4-year term averages, residualized on city and year fixed-effects. Per-capita variables are taken in logarithms. Fitted lines are estimated semi-parametrically through kernel-weighted local linear regression (triangular kernel), with MSE-optimal bandwidth.

Figure 3.2: Effect of a left-wing mayor on the share of social spending, by year in office



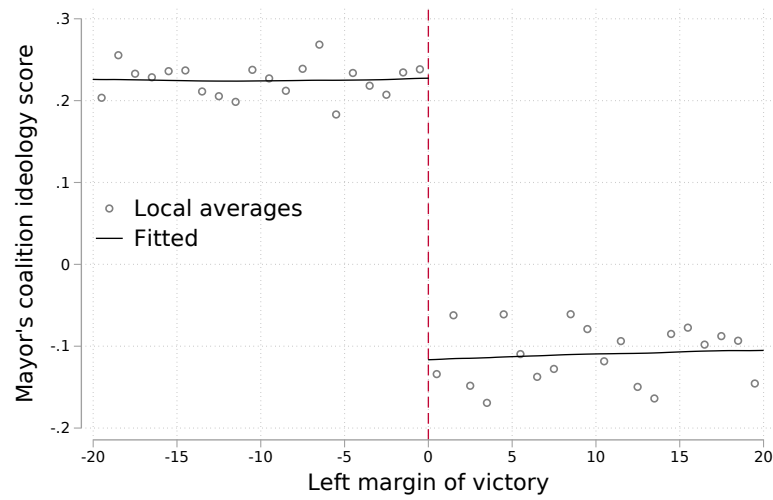
Notes: Effect of a left-wing mayor on the share of social spending from our RD specification (equation 3.2), using the robust and bias-corrected procedure of Calonico et al. [2014] and controlling for city and year fixed effects. Bars represent 95% confidence intervals from robust bias-corrected standard errors clustered by municipality.

Figure 3.3: Falsification test using placebo thresholds for the effect of a left-wing mayor on social expenditures



Notes: Empirical distribution of t-statistics from our RD estimates (equation 3.2) of the effect of a left-wing mayor on the share of social spending and social expenditure per capita, based on 200 randomly-drawn placebo thresholds, drawn separately on the left and on the right side of the true threshold (100 on each side), using only observations belonging to that side and with at least 25 observations on each side of the bandwidth. Vertical line = t-statistics obtained using the true threshold. The t-statistics are from the robust bias-corrected procedure of Calonico et al. [2014].

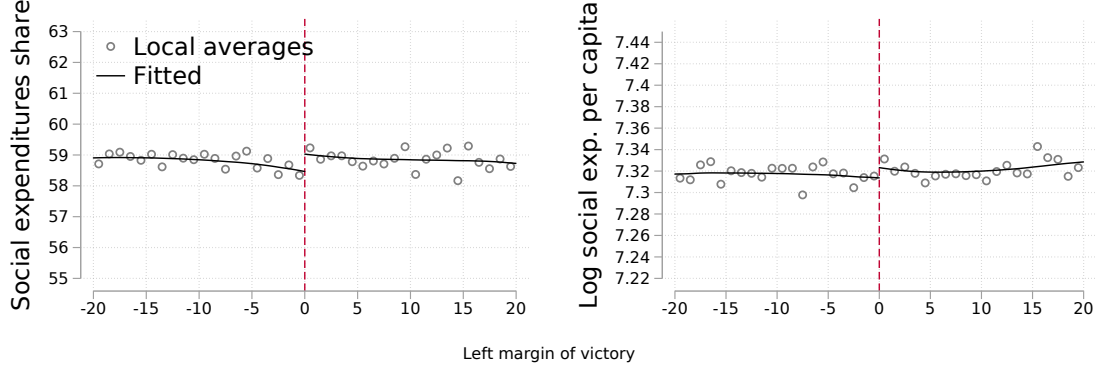
Figure 3.4: Ideology score for the coalition of the elected mayor



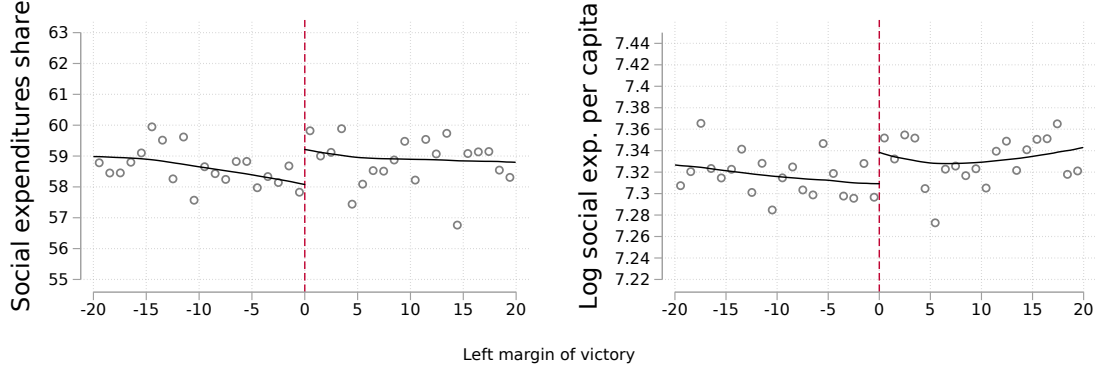
Notes: Left-right ideology score for the coalition supporting the elected mayor on the vertical axis (higher values indicate more right-wing coalitions). Margin of the left-wing candidate on the horizontal axis. See main text for the definition and construction of these two variables. Fitted lines are estimated semi-parametrically through kernel-weighted local linear regression (triangular kernel) with MSE-optimal bandwidth.

Figure 3.5: Effect of a left-wing mayor on social spending

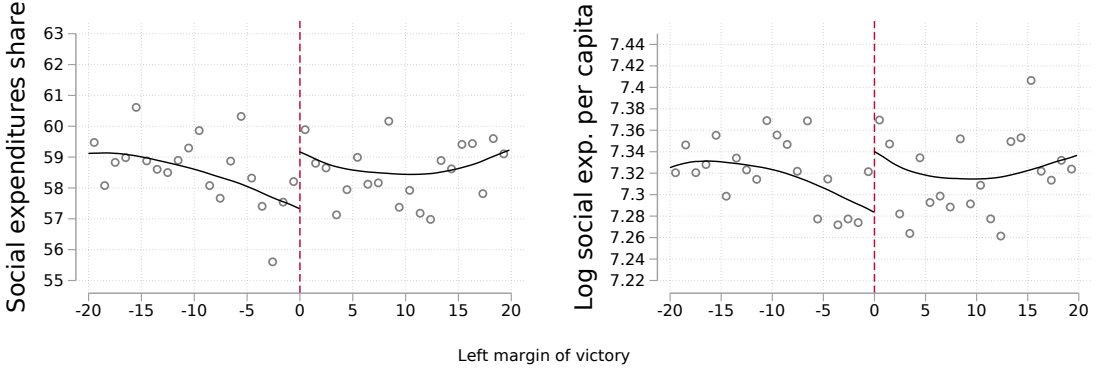
(a) *Whole sample*



(b) *Cities with lame duck mayor*



(c) *Cities experiencing oil windfalls*



Notes: Visual presentation of our RD estimates of the effect of a left-wing mayor on social expenditures per capita (left) and as share of total expenditures (right) for each subsample, as reported in columns 1, 2 and 5 of table 3.3 and based on the specification in equation 3.2. All outcomes are 4-year term averages, residualized on city and year fixed-effects. Per-capita variables are taken in logarithms. Fitted lines are estimated semi-parametrically through kernel-weighted local linear regression (triangular kernel), with MSE-optimal bandwidth. See main text for definition and interpretation of the subsamples.

Table 3.4: Differential effect on social expenditures in subsamples, relative to the rest of the sample

Outcome	Subsample					
	Lame duck	Oil Windfall	Tiebout competition		Ideology distance	
			< median	< 25th pct	> median	> 75th pct
Social expenditures share	0.91 (0.48)	1.74 (1.01)	0.41 (0.42)	-0.42 (0.47)	0.56 (0.50)	0.38 (0.59)
Social expenditures per capita	3.09 (1.72)	5.74 (2.74)	-1.40 (1.39)	-2.24 (1.57)	0.75 (1.52)	-1.02 (1.75)

Notes: For each subsample, this table reports the difference between the estimated effect in the subsample and in the rest of the sample. In each subsample, estimates are obtained from our baseline RD specification (equation 3.2), using the bias-corrected procedure of Calonico et al. [2014] and controlling for city and year fixed effects. Standard errors clustered by municipality are obtained from 500 bootstrap replications. See Section 3.6.5 for more details on the procedure.

APPENDIX A
ADDITIONAL RESULTS OF CHAPTER 1

Pricing Framework

In this section, I present a simple pricing framework to help us understand the main sources of heterogeneity in pass-through for consumer prices across different product categories. I focus on presenting reduced-form pricing equations, even though they can be derived from structural models.¹ Moreover, this simple framework is in partial equilibrium in the sense that wages, employment and the exchange rate are taken as given.

Let us assume there are three sectors in the economy: tradable (T), non-tradable (N) and retail (R) sectors. The tradable sector uses imported and local inputs in a constant returns to scale technology, where α is the share of imported inputs used in production. Retailers, in turn, combine tradable goods and distribution services (D) – a non-tradable good – to sell goods to consumers. Both sectors produce differentiated products and operate under monopolistic competition, charging a markup over marginal costs. The non-tradable sector produces a homogeneous good.²

As discussed before and presented in figure 1.2 for the case of Brazil, prices at the dock track closely the nominal exchange rate. Then, according to the LOP, prices of imported goods are given by:

$$P^I = E \tag{A.1}$$

¹For a review of these models, see Burstein and Gopinath [2015]

²To avoid clutter, I do not use an index for each product in the following equations, but we should keep in mind that each equation holds for all product categories.

where the price of the foreign good was normalized to 1 and E is the nominal exchange rate (local currency/foreign currency).

Since firms at the tradable sector combine imported and local inputs, prices of tradable goods are given by:

$$P^T = \mu^T \left(E^\alpha P^L^{(1-\alpha)} \right) \quad (\text{A.2})$$

where P^T is the price of tradable goods, P^L is the price of local inputs, μ^T is the markup of the tradable producer and α is the share of imported goods in production.

To sell goods to consumers, the retail sector combines tradable goods with distribution services, which is a non-tradable good. Since the non-tradable good is assumed to be homogeneous, $P^D = P^N$ and the retail price is given by:

$$P^R = \mu^R \left(P^{T^\theta} P^{N(1-\theta)} \right) \quad (\text{A.3})$$

where $1 - \theta$ is the share of distribution services on tradable prices and μ^R is the markup of the retail sector. Plugging in (A.2) into (A.3) and log-differentiating, we get:

$$\hat{P}^R = \hat{\mu}^R + \theta \hat{\mu}^T + \theta \alpha \hat{E} + \theta(1 - \alpha) \hat{P}^L + (1 - \theta) \hat{P}^N \quad (\text{A.4})$$

Equation (A.4) highlights the potential mechanisms that make exchange rate pass-through different across products: retail markups $\hat{\mu}^R$, the share of distribution services in

retail prices θ , tradable producer markups $\hat{\mu}^T$ and the share of imported inputs in tradable production α .³

If markups ($\hat{\mu}^T$ and $\hat{\mu}^R$) respond to the exchange rate shocks differently across products, the price changes following a devaluation will also vary across product categories. In this case, inflation will be higher for products whose markup rates increase by a greater extent. This mechanism is hard to assess empirically given that data on markup rates at the product level is hard to obtain, but Gopinath et al. [2011] were able to assess how retailers markup respond to the exchange rate. Using a unique data set on prices and wholesale costs from a large retail chain that operates in the United States and Canada, the authors decompose the variation in cross-border retail prices into relative costs and markup components. They show that “almost all of the variation in relative retail prices, in response to exchange rate shocks, is explained by variation in relative costs (net or wholesale) and not by variation in relative markups”[Gopinath et al., 2011, p. 2461]. Therefore, this channel seems less relevant and we can simplify by assuming $\hat{\mu}^R = 0$ for all products.

The role of distribution services in explaining incomplete pass-through to consumer prices was already discussed in the previous section[Burstein et al., 2003, 2007, Burstein and Gopinath, 2015]. Here, the intuition is the same, but with a focus on comparing different products. Since distribution services are non-tradables and the relative price of tradable to non-tradables (then $\hat{P}^T > \hat{P}^N$) increases after a devaluation, products with

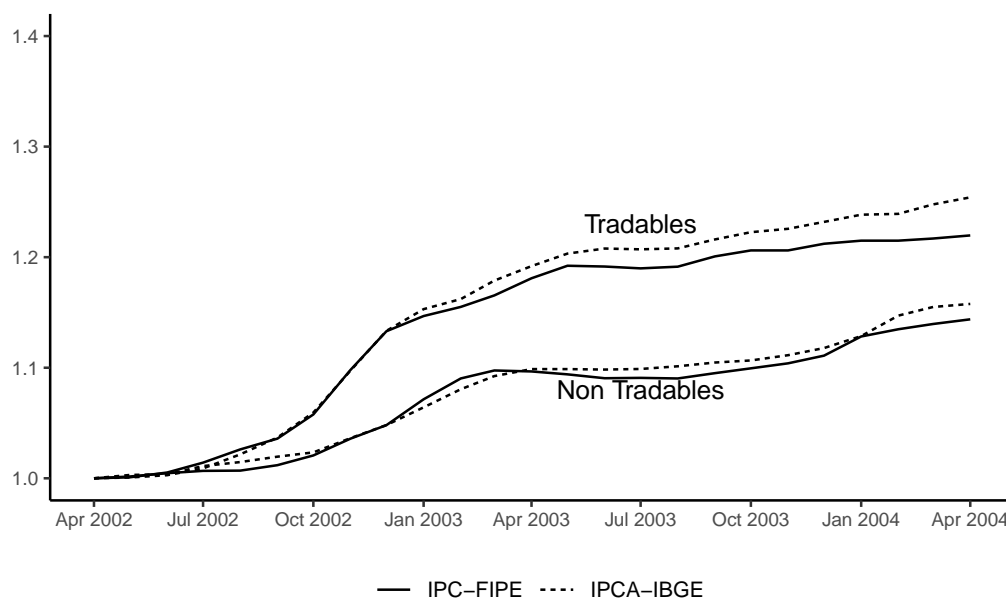
³I assumed throughout that the prices of local inputs are not affected by the nominal exchange rate.

a smaller share of distribution services (high θ) will have a higher inflation rate after the devaluation.

Finally, the import content of each product also affects pass-through to consumer prices. Since the law of one price seems to hold for prices at the dock, local prices of imported goods adjust completely to the exchange rate shock. Therefore, the higher the share of imported goods in a product category (α) the higher its price change after the devaluation.

IPCA-IBGE *vs* IPC-Fipe

Figure A.1: Comparison between the evolution of IPCA and IPC-Fipe after the devaluation shock



Notes: Price of tradables and nontradables from IPCA and IPC-Fipe. All indices are normalized to 1 in April 2002, the month before the devaluation. Source: Author's calculation using CPI data from IBGE and FIPE.

A.1 Descriptive Statistics

Table A.1: Average Income and expenditure shares across broad consumption categories

	Deciles									
	1	2	3	4	5	6	7	8	9	10
Brazil: IBGE Data										
Average Income	184.83	321.13	436.90	558.93	700.37	886.18	1152.27	1579.58	2441.03	7507.11
Food and Beverages	0.40	0.39	0.37	0.36	0.32	0.30	0.28	0.25	0.20	0.14
Housing	0.22	0.20	0.19	0.18	0.19	0.17	0.16	0.15	0.13	0.10
Household Fur. and App.	0.08	0.07	0.08	0.08	0.07	0.08	0.07	0.07	0.06	0.05
Aparel	0.07	0.08	0.08	0.08	0.08	0.09	0.09	0.09	0.08	0.07
Transportation	0.08	0.10	0.10	0.10	0.14	0.16	0.18	0.21	0.27	0.32
Health	0.08	0.08	0.09	0.09	0.09	0.09	0.09	0.09	0.08	0.08
Individual Expenses	0.04	0.04	0.04	0.05	0.05	0.06	0.06	0.07	0.08	0.11
Education	0.01	0.01	0.01	0.01	0.02	0.02	0.03	0.03	0.05	0.09
Communication	0.02	0.02	0.03	0.03	0.04	0.04	0.05	0.05	0.05	0.04
City of São Paulo: FIPE Data										
Average Income	338.92	668.54	939.61	1200.94	1502.64	1878.76	2427.72	3313.69	4842.16	10421.00
Food and Beverages	0.30	0.26	0.26	0.21	0.21	0.22	0.18	0.16	0.13	0.12
Housing	0.35	0.38	0.36	0.40	0.33	0.31	0.34	0.32	0.32	0.30
Aparel	0.04	0.04	0.04	0.04	0.04	0.05	0.04	0.04	0.04	0.05
Transportation	0.10	0.13	0.15	0.15	0.21	0.19	0.20	0.20	0.22	0.21
Health	0.06	0.05	0.05	0.08	0.07	0.07	0.07	0.10	0.10	0.11
Individual Expenses	0.14	0.13	0.12	0.11	0.11	0.13	0.11	0.11	0.11	0.12
Education	0.00	0.00	0.01	0.01	0.03	0.04	0.04	0.06	0.07	0.10

Note: Panel A shows monthly average income and expenditure shares across the deciles of income distribution using data from IBGE, while Panel B shows the same information using data from FIPE. Monetary values are in Reais of January, 2003. Source: Author's calculation using data from Consumer Expenditure Surveys (POF/IBGE 2002-2003 and POF/FIPE 1998-1999).

Unit value by income in the City of São Paulo

In the consumer expenditure survey from FIPE (POF 1998-1999), I can observe unit value paid by households for each good. Using these unit prices, I estimate the following model:

$$\ln u_g^h = c + \sum_{g \in G} \alpha_g + \sum_{j=2}^{10} \beta_j I[h \in Dec(j)] + e_g^h \quad (\text{A.5})$$

where u_g^h is the unit value paid by household h for a good $g \in G$, $I[h \in Dec(j)]$ is an indicator if household h belongs to income decile j , α_g 's are IPC subitem dummies to control for specific characteristics of each product and e_g^h is the residual term.

The results reported in table A.2 show that the higher the household income the higher the price paid for goods within a product category as the decile dummies become increasingly positive and significant for higher deciles. Comparing the top and bottom deciles, the richest households in São Paulo paid on average 0.34 log points higher prices than the poorest households.

Table A.2: Unit value by income in the City of São Paulo

Deciles	Estimate
decile2	0.0153 (0.008)
decile3	0.0173 (0.008)
decile4	0.0633 (0.008)
decile5	0.0683 (0.008)
decile6	0.0653 (0.008)
decile7	0.113 (0.008)
decile8	0.1433 (0.008)
decile9	0.2193 (0.008)
decile10	0.3383 (0.007)
N	216902
Adj.R2	0.78

Notes: Robust standard errors in parentheses. The specification includes product fixed effects. Source: Author's calculation using data from Consumer Expenditure Survey of the City of São Paulo (POF/FIPE 1998-1999)

Robustness: Across Price Index

Table A.3: The Across price indices by region

	Deciles											1st/10th ratio
	1	2	3	4	5	6	7	8	9	10	CPI	
Bahia												
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.243	1.231	1.248	1.228	1.199	1.211	1.198	1.192	1.171	1.139	1.185	1.752
2004-01-01	1.307	1.302	1.317	1.306	1.265	1.285	1.269	1.250	1.220	1.178	1.241	1.727
Ceará												
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.260	1.247	1.239	1.219	1.226	1.213	1.199	1.196	1.160	1.150	1.188	1.730
2004-01-01	1.305	1.308	1.297	1.280	1.287	1.274	1.260	1.254	1.222	1.194	1.241	1.573
Minas Gerais												
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.234	1.201	1.200	1.207	1.200	1.188	1.189	1.169	1.163	1.123	1.161	1.893
2004-01-01	1.295	1.257	1.258	1.241	1.247	1.240	1.237	1.210	1.205	1.160	1.218	1.848
Pará												
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.235	1.219	1.221	1.203	1.208	1.195	1.185	1.184	1.157	1.153	1.174	1.531
2004-01-01	1.292	1.280	1.278	1.259	1.271	1.250	1.253	1.250	1.217	1.207	1.240	1.408
Pernambuco												
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.223	1.229	1.216	1.216	1.185	1.198	1.182	1.183	1.164	1.137	1.176	1.622
2004-01-01	1.272	1.281	1.265	1.260	1.249	1.263	1.237	1.237	1.225	1.192	1.231	1.414
Paraná												
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.200	1.190	1.189	1.172	1.198	1.164	1.164	1.154	1.156	1.141	1.160	1.421
2004-01-01	1.241	1.224	1.221	1.203	1.223	1.190	1.188	1.173	1.175	1.164	1.193	1.467
Rio de Janeiro												
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.219	1.230	1.207	1.214	1.212	1.210	1.189	1.173	1.171	1.159	1.175	1.376
2004-01-01	1.278	1.293	1.270	1.274	1.271	1.263	1.238	1.210	1.213	1.190	1.224	1.466
Rio Grande do Sul												
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.234	1.197	1.216	1.185	1.206	1.187	1.166	1.172	1.155	1.165	1.171	1.424
2004-01-01	1.258	1.241	1.254	1.225	1.243	1.221	1.205	1.209	1.194	1.196	1.211	1.319
São Paulo												
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.203	1.176	1.187	1.184	1.170	1.162	1.150	1.148	1.133	1.140	1.156	1.455
2004-01-01	1.242	1.214	1.231	1.224	1.205	1.188	1.187	1.181	1.161	1.169	1.190	1.437

Notes: Column Aggregate refers to the across price index using economy-wide weights. Column CPI reports the actual figures from IPCA-IBGE and IPC-FIPE. Column 1st/10th ratio refers to the accumulated inflation ratio between households in the first (poor) and tenth (rich) deciles. Source: Author's calculation.

Table A.4: The Across price indices by income decile using end-of-period weights

	Deciles										Aggregate	CPI	1st/10th ratio	
	1	2	3	4	5	6	7	8	9	10				
Panel A: group level														
Brazil														
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.177	1.178	1.175	1.175	1.174	1.172	1.170	1.168	1.164	1.154	1.164	1.168	1.150	
2004-01-01	1.225	1.225	1.222	1.222	1.221	1.219	1.217	1.214	1.211	1.203	1.212	1.211	1.106	
Panel B: subitem level														
Brazil														
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000	
2003-04-01	1.220	1.220	1.209	1.209	1.200	1.197	1.189	1.182	1.175	1.158	1.179	1.168	1.390	
2004-01-01	1.275	1.273	1.259	1.258	1.251	1.246	1.239	1.227	1.219	1.202	1.225	1.211	1.363	

Notes: Column Aggregate refers to the across price index using economy-wide weights. Column CPI reports the actual figures from IPCA-IBGE and IPC-FIPE. Column 1st/10th ratio refers to the accumulated inflation ratio between households in the first (poor) and tenth (rich) deciles. Source: Author's calculation.

Robustness: Within and Combined Price Indices

Table A.5: The Within price indices using restricted definition of individual goods

	Conservative				Liberal			
	Below Median	Above Median	Quartile 1	Quartile 4	Below Median	Above Median	Quartile 1	Quartile 4
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
2003-04-01	1.162	1.140	1.167	1.131	1.207	1.147	1.220	1.123
2004-01-01	1.219	1.191	1.224	1.181	1.278	1.201	1.291	1.175

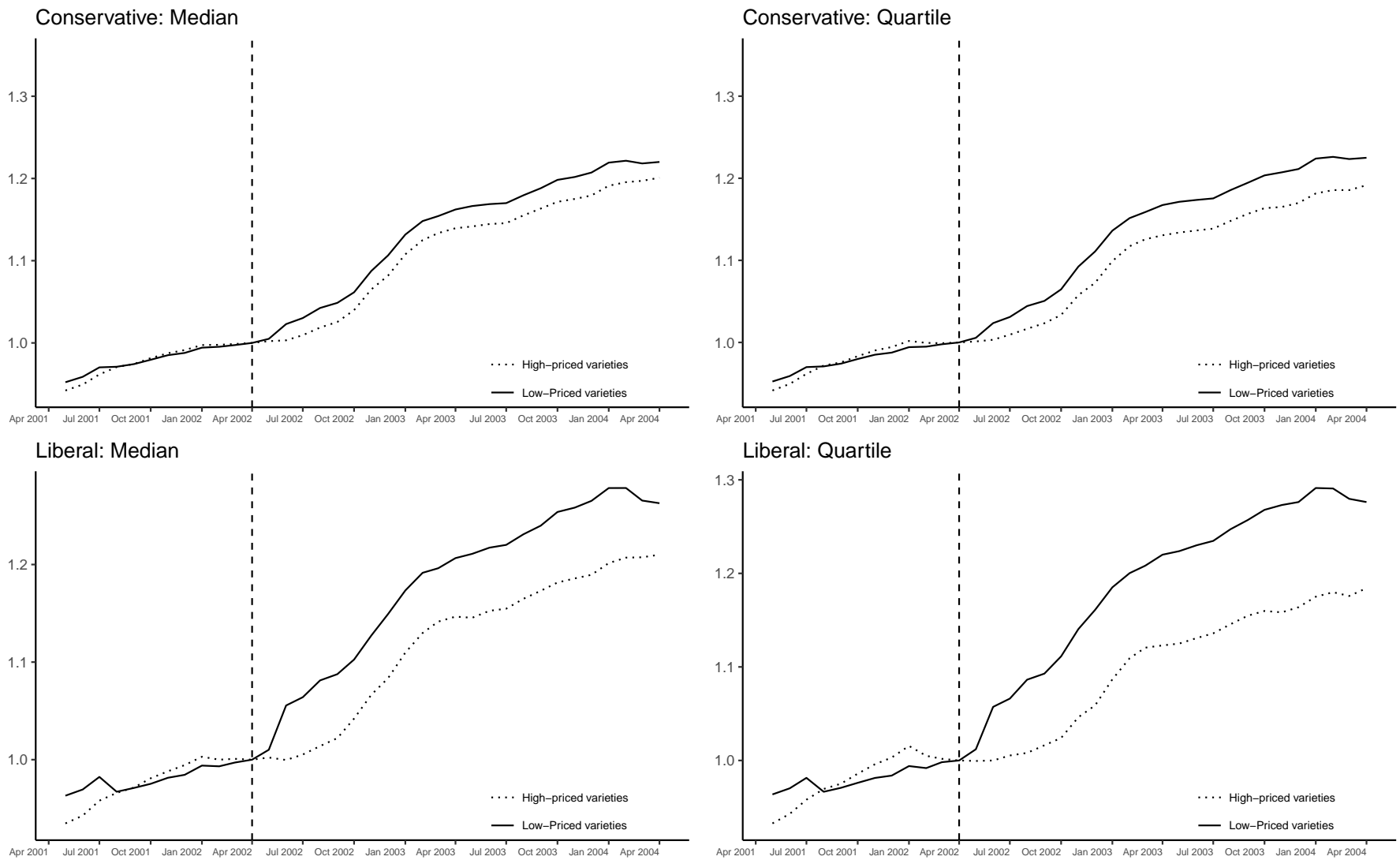
Note: The household-specific within price index is computed as a weighted average of economy-wide expenditure shares (ω_g) and household-specific price indexes for each product category (P_g^h): $\hat{P}_{Within,t} \equiv \sum_{g \in G} \omega_g \hat{P}_{g,t}^h$

Table A.6: The Combined price indices using restricted definition of individual goods

	Conservative				Liberal			
	Below Median	Above Median	Quartile 1	Quartile 4	Below Median	Above Median	Quartile 1	Quartile 4
2002-04-01	1.000	1.000	1.000	1.000	1.000	1.000	1.000	1.000
2003-04-01	1.242	1.122	1.253	1.116	1.295	1.131	1.317	1.112
2004-01-01	1.291	1.180	1.303	1.173	1.356	1.188	1.378	1.166

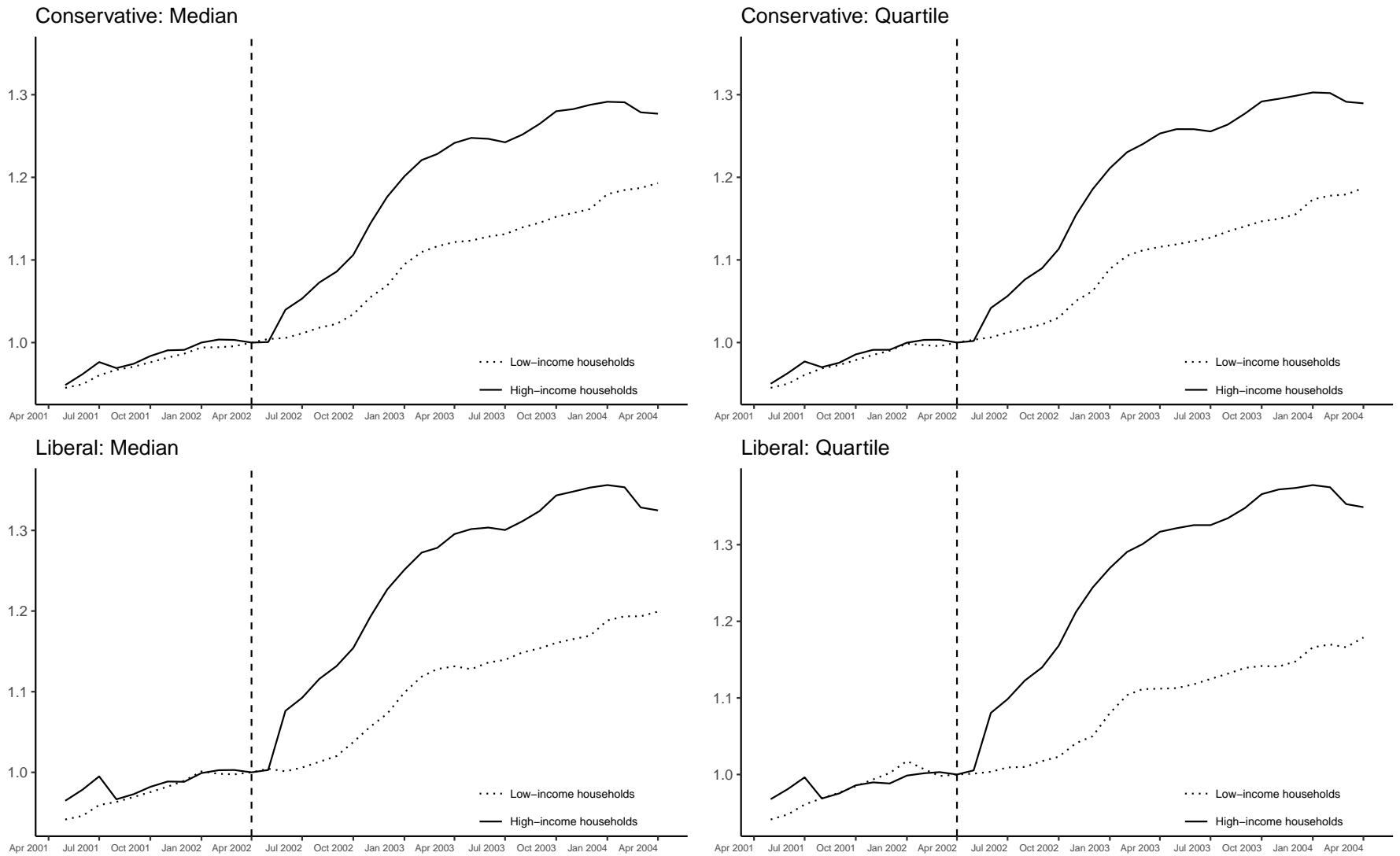
Note: The household-specific within price index is computed as a weighted average of economy-wide expenditure shares (ω_g) and household-specific price indexes for each product category (P_g^h): $\hat{P}_{Within,t} \equiv \sum_{g \in G} \omega_g \hat{P}_{g,t}^h$

Figure A.2: Within price indices using restricted definition of individual goods



Note: The household-specific within price index is computed as a weighted average of economy-wide expenditure shares (ω_g) and household-specific price indexes for each product category (P_g^h): $\hat{P}_{Within,t}^h \equiv \sum_{g \in G} \omega_g \hat{P}_{g,t}^h$

Figure A.3: Combined price indices indices using restricted definition of individual goods



Note: The household-specific within price index is computed as a weighted average of economy-wide expenditure shares (ω_g) and household-specific price indexes for each product category (P_g^h): $\hat{P}_{Within,t}^h \equiv \sum_{g \in G} \omega_g \hat{P}_{g,t}^h$

APPENDIX B

ADDITIONAL RESULTS OF CHAPTER 2

Robustness tests

Table B.1: Employment effects of real exchange rate shocks - estimated separate

	Export-weighted RER				Import-weighted RER			
	Baseline		Estimated separate		Baseline		Estimated separate	
	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)
All sectors								
Contemporaneous effect	0.260** (0.128)	0.265** (0.106)	0.299*** (0.115)	0.321*** (0.111)	-0.039 (0.058)	-0.056 (0.063)	-0.041 (0.050)	-0.052 (0.055)
Short-run effect	0.264 (0.234)	0.269* (0.160)	0.395* (0.204)	0.430*** (0.143)	-0.227* (0.129)	-0.257** (0.111)	-0.208** (0.094)	-0.227*** (0.080)
Medium-run effect	0.774*** (0.227)	0.823*** (0.246)	0.695*** (0.210)	0.776*** (0.227)	-0.054 (0.125)	-0.130 (0.104)	-0.147 (0.125)	-0.207** (0.097)
Tradable sector								
Contemporaneous effect	0.342 (0.227)	0.354* (0.209)	0.366** (0.186)	0.393** (0.188)	-0.051 (0.118)	-0.071 (0.096)	-0.091 (0.094)	-0.102 (0.076)
Short-run effect	0.506* (0.262)	0.523** (0.216)	0.628** (0.276)	0.670*** (0.231)	-0.289* (0.158)	-0.325** (0.133)	-0.365** (0.149)	-0.384*** (0.121)
Medium-run effect	1.044*** (0.353)	1.115*** (0.319)	0.903*** (0.343)	1.000*** (0.295)	0.022 (0.172)	-0.062 (0.158)	-0.178 (0.182)	-0.238 (0.146)
Nontradable sector								
Contemporaneous effect	0.106 (0.127)	0.095 (0.123)	0.160 (0.109)	0.156 (0.120)	-0.046 (0.060)	-0.048 (0.075)	-0.014 (0.046)	-0.014 (0.061)
Short-run effect	0.174 (0.154)	0.151 (0.123)	0.287** (0.127)	0.276** (0.120)	-0.136 (0.096)	-0.140 (0.105)	-0.082 (0.063)	-0.080 (0.075)
Medium-run effect	0.374 (0.252)	0.338 (0.326)	0.346 (0.233)	0.323 (0.303)	-0.124 (0.094)	-0.140 (0.097)	-0.122 (0.100)	-0.127 (0.104)
Manufacturing sector								
Contemporaneous effect	0.753* (0.425)	0.775** (0.388)	0.813** (0.347)	0.829** (0.342)	-0.147 (0.242)	-0.151 (0.220)	-0.285 (0.194)	-0.275 (0.200)
Short-run effect	0.724* (0.376)	0.750** (0.335)	0.832** (0.377)	0.846** (0.331)	-0.473** (0.235)	-0.478** (0.189)	-0.596*** (0.221)	-0.577*** (0.195)
Medium-run effect	1.665*** (0.417)	1.759*** (0.411)	1.736*** (0.447)	1.790*** (0.423)	-0.444* (0.265)	-0.452** (0.224)	-0.805*** (0.261)	-0.776*** (0.219)
Primary sector								
Contemporaneous effect	-0.020 (0.274)	0.006 (0.296)	0.104 (0.253)	0.157 (0.262)	-0.151 (0.186)	-0.192 (0.201)	-0.116 (0.137)	-0.154 (0.161)
Short-run effect	0.318 (0.204)	0.369 (0.289)	0.625*** (0.212)	0.718*** (0.239)	-0.395 (0.270)	-0.471* (0.252)	-0.450* (0.249)	-0.523** (0.210)
Medium-run effect	0.596 (0.417)	0.700 (0.506)	0.496 (0.414)	0.675 (0.444)	0.018 (0.245)	-0.171 (0.244)	-0.066 (0.274)	-0.245 (0.231)
Year fixed effect	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effect	Y		Y		Y		Y	
Meso-region fixed effect		Y		Y		Y		Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
SE clustered by state	Y		Y		Y		Y	
SE clustered by meso-region		Y		Y		Y		Y

Notes: The dependent variable is measured as 100 times the log change of formal employment. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by exports or imports of each Brazilian trade partner and the sectoral composition of employment. The export-based and import-base RER are included separate in each estimation. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration obtained from the 1991 Census. Clustered-robust standard-errors in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.10.

Table B.2: Employment effects of real exchange rate shocks by different level of sectoral aggregation

	Change in employment			
	3 digits		2 digits	
	(3)	(5)	(3)	(5)
Export-weighted RER				
Contemporaneous effect	0.260** (0.128)	0.265** (0.106)	0.395* (0.224)	0.421** (0.190)
Short-run effect	0.264 (0.234)	0.269* (0.160)	0.522** (0.205)	0.608*** (0.188)
Medium-run effect	0.774*** (0.227)	0.823*** (0.246)	0.552* (0.318)	0.625** (0.305)
Import-weighted RER				
Contemporaneous effect	-0.039 (0.058)	-0.056 (0.063)	-0.056 (0.103)	-0.117 (0.107)
Short-run effect	-0.227* (0.129)	-0.257** (0.111)	-0.232 (0.191)	-0.336** (0.154)
Medium-run effect	-0.054 (0.125)	-0.130 (0.104)	-0.188 (0.216)	-0.397** (0.172)
Year fixed effect	Y	Y	Y	Y
State fixed effect	Y		Y	
Meso-region fixed effect		Y		Y
Controls	Y	Y	Y	Y
SE clustered by state	Y		Y	
SE clustered by meso-region		Y		Y

Notes: The dependent variable is measured as 100 times the log change of formal employment in each sector. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by exports or imports of each Brazilian trade partner and the sectoral composition of employment. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration. Clustered-robust standard-errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B.3: Employment effects of real exchange rate shocks by different labor shares criteria

	Change in employment					
	Baseline		Lagged 1 year		Lagged 2 years	
	3-year moving avg.					
	(3)	(5)	(3)	(5)	(3)	(5)
Export-weighted RER						
Contemporaneous effect	0.260** (0.128)	0.265** (0.106)	0.152 (0.220)	0.146 (0.153)	0.032 (0.178)	0.034 (0.195)
Short-run effect	0.264 (0.234)	0.269* (0.160)	0.259 (0.310)	0.254 (0.272)	0.281 (0.192)	0.292 (0.202)
Medium-run effect	0.774*** (0.227)	0.823*** (0.246)	0.649*** (0.247)	0.660*** (0.231)	0.568** (0.274)	0.607*** (0.230)
Import-weighted RER						
Contemporaneous effect	-0.039 (0.058)	-0.056 (0.063)	-0.041 (0.050)	-0.046 (0.058)	-0.191** (0.083)	-0.206*** (0.065)
Short-run effect	-0.227* (0.129)	-0.257** (0.111)	-0.231* (0.121)	-0.242** (0.109)	-0.241** (0.100)	-0.271*** (0.078)
Medium-run effect	-0.054 (0.125)	-0.130 (0.104)	-0.054 (0.133)	-0.093 (0.112)	-0.113 (0.121)	-0.191* (0.102)
Year fixed effect	Y	Y	Y	Y	Y	Y
State fixed effect	Y		Y		Y	
Meso-region fixed effect		Y		Y		Y
Controls	Y	Y	Y	Y	Y	Y
SE clustered by state	Y		Y		Y	
SE clustered by meso-region		Y		Y		Y

Notes: The dependent variable is measured as 100 times the log change of formal employment in each sector. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by exports or imports of each Brazilian trade partner and the sectoral composition of employment. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration. Clustered-robust standard-errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B.4: Employment effects of real exchange rate shocks controlling for confounding shocks: tradable sector

	Change in sectoral employment							
	Baseline				Baseline + Confounders			
	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)
Export-weighted RER								
Contemporaneous effect	0.342 (0.227)	0.354* (0.209)	0.444* (0.253)	0.475** (0.221)	0.342 (0.228)	0.358* (0.210)	0.445* (0.253)	0.481** (0.221)
Short-run effect	0.506* (0.262)	0.523** (0.216)	0.625*** (0.221)	0.670*** (0.201)	0.506* (0.266)	0.532** (0.218)	0.626*** (0.221)	0.681*** (0.202)
Medium-run effect	1.044*** (0.353)	1.115*** (0.319)	1.216*** (0.313)	1.352*** (0.310)	1.043*** (0.358)	1.133*** (0.323)	1.220*** (0.309)	1.374*** (0.313)
Import-weighted RER								
Contemporaneous effect	-0.051 (0.118)	-0.071 (0.096)	-0.020 (0.115)	-0.039 (0.102)	-0.051 (0.118)	-0.074 (0.096)	-0.021 (0.115)	-0.042 (0.102)
Short-run effect	-0.289* (0.158)	-0.325** (0.133)	-0.253* (0.149)	-0.288** (0.130)	-0.289* (0.156)	-0.330** (0.132)	-0.254* (0.147)	-0.294** (0.128)
Medium-run effect	0.022 (0.172)	-0.062 (0.158)	0.146 (0.161)	0.065 (0.161)	0.023 (0.170)	-0.074 (0.154)	0.144 (0.160)	0.055 (0.158)
Confounders								
Commodity price shocks			-2.554 (1.876)	-0.598 (2.146)			-2.567 (1.893)	-0.702 (2.141)
Trade liberalization shock					-0.407 (9.769)	16.692 (11.461)	1.846 (9.902)	17.696 (13.768)
Year fixed effect	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effect	Y		Y		Y		Y	
Meso-region fixed effect		Y		Y		Y		Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
SE clustered by state	Y		Y		Y		Y	
SE clustered by meso-region		Y		Y		Y		Y

Notes: The dependent variable is measured as 100 times the log change of overall formal employment. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by sectoral exports or imports of each Brazilian trade partner and the sectoral composition of employment at the local labor market. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration in the 1991 Census. The commodity price and trade liberalization shock controls are from Adao [2016] and Dix-Carneiro and Kovak [2017]. Clustered-robust standard-errors in parentheses.

*** p < 0.01, ** p < 0.5, * p < 0.10.

Table B.5: Employment effects of real exchange rate shocks controlling for confounding shocks: nontradable sector

	Change in sectoral employment							
	Baseline		Baseline + Confounders					
	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)
Export-weighted RER								
Contemporaneous effect	0.106 (0.127)	0.095 (0.123)	0.094 (0.129)	0.098 (0.142)	0.105 (0.127)	0.095 (0.123)	0.092 (0.130)	0.096 (0.141)
Short-run effect	0.174 (0.154)	0.151 (0.123)	0.218 (0.158)	0.217* (0.130)	0.172 (0.155)	0.151 (0.124)	0.215 (0.160)	0.214* (0.130)
Medium-run effect	0.374 (0.252)	0.338 (0.326)	0.359 (0.280)	0.372 (0.369)	0.370 (0.252)	0.338 (0.325)	0.352 (0.276)	0.367 (0.365)
Import-weighted RER								
Contemporaneous effect	-0.046 (0.060)	-0.048 (0.075)	-0.036 (0.063)	-0.055 (0.081)	-0.045 (0.060)	-0.048 (0.075)	-0.035 (0.063)	-0.054 (0.081)
Short-run effect	-0.136 (0.096)	-0.140 (0.105)	-0.121 (0.103)	-0.157 (0.123)	-0.135 (0.096)	-0.140 (0.105)	-0.119 (0.103)	-0.156 (0.122)
Medium-run effect	-0.124 (0.094)	-0.140 (0.097)	-0.120 (0.099)	-0.211* (0.116)	-0.121 (0.096)	-0.140 (0.097)	-0.116 (0.102)	-0.209* (0.115)
Confounders								
Commodity price shocks			3.661** (1.521)	5.406** (2.355)			3.688** (1.551)	5.432** (2.378)
Trade liberalization shock					-2.856 (9.416)	0.107 (5.615)	-3.812 (11.240)	-4.380 (9.105)
Year fixed effect	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effect	Y		Y		Y		Y	
Meso-region fixed effect		Y		Y		Y		Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
SE clustered by state	Y		Y		Y		Y	
SE clustered by meso-region		Y		Y		Y		Y

Notes: The dependent variable is measured as 100 times the log change of overall formal employment. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by sectoral exports or imports of each Brazilian trade partner and the sectoral composition of employment at the local labor market. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration in the 1991 Census. The commodity price and trade liberalization shock controls are from Adao [2016] and Dix-Carneiro and Kovak [2017]. Clustered-robust standard-errors in parentheses.

*** p < 0.01, ** p < 0.05, * p < 0.10.

Table B.6: Employment effects of real exchange rate shocks controlling for confounding shocks: manufacturing sector

	Change in sectoral employment							
	Baseline				Baseline + Confounders			
	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)
Export-weighted RER								
Contemporaneous effect	0.753* (0.425)	0.775** (0.388)	0.971** (0.423)	1.030*** (0.373)	0.746* (0.426)	0.776** (0.388)	0.965** (0.423)	1.032*** (0.373)
Short-run effect	0.724* (0.376)	0.750** (0.335)	0.875*** (0.320)	0.944*** (0.306)	0.709* (0.383)	0.754** (0.338)	0.861*** (0.323)	0.947*** (0.308)
Medium-run effect	1.665*** (0.417)	1.759*** (0.411)	1.785*** (0.410)	1.975*** (0.453)	1.634*** (0.429)	1.766*** (0.419)	1.751*** (0.412)	1.981*** (0.460)
Import-weighted RER								
Contemporaneous effect	-0.147 (0.242)	-0.151 (0.220)	-0.119 (0.247)	-0.111 (0.227)	-0.143 (0.242)	-0.152 (0.220)	-0.114 (0.248)	-0.112 (0.227)
Short-run effect	-0.473** (0.235)	-0.478** (0.189)	-0.454* (0.240)	-0.442** (0.197)	-0.465** (0.234)	-0.480** (0.188)	-0.445* (0.240)	-0.443** (0.195)
Medium-run effect	-0.444* (0.265)	-0.452** (0.224)	-0.426 (0.306)	-0.401 (0.268)	-0.425 (0.264)	-0.456** (0.219)	-0.408 (0.306)	-0.404 (0.262)
Confounders								
Commodity price shocks			-0.117 (4.760)	2.455 (6.108)			0.018 (4.771)	2.428 (6.115)
Trade liberalization shock					-19.290 (13.205)	6.219 (17.102)	-18.799 (14.357)	4.634 (20.467)
Year fixed effect	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effect	Y		Y		Y		Y	
Meso-region fixed effect		Y		Y		Y		Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
SE clustered by state	Y		Y		Y		Y	
SE clustered by meso-region		Y		Y		Y		Y

Notes: The dependent variable is measured as 100 times the log change of overall formal employment. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by sectoral exports or imports of each Brazilian trade partner and the sectoral composition of employment at the local labor market. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration in the 1991 Census. The commodity price and trade liberalization shock controls are from Adao [2016] and Dix-Carneiro and Kovak [2017] Clustered-robust standard-errors in parentheses.

*** p < 0.01, ** p < 0.5, * p < 0.10.

Table B.7: Employment effects of real exchange rate shocks controlling for confounding shocks: primary sector

	Change in sectoral employment							
	Baseline				Baseline + Confounders			
	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)
Export-weighted RER								
Contemporaneous effect	-0.020 (0.274)	0.006 (0.296)	0.083 (0.307)	0.106 (0.347)	-0.015 (0.274)	0.012 (0.296)	0.089 (0.307)	0.115 (0.348)
Short-run effect	0.318 (0.204)	0.369 (0.289)	0.372* (0.192)	0.436 (0.349)	0.330 (0.209)	0.380 (0.292)	0.387** (0.195)	0.453 (0.355)
Medium-run effect	0.596 (0.417)	0.700 (0.506)	0.641* (0.361)	0.773 (0.577)	0.621 (0.429)	0.721 (0.509)	0.676* (0.371)	0.808 (0.586)
Import-weighted RER								
Contemporaneous effect	-0.151 (0.186)	-0.192 (0.201)	-0.111 (0.187)	-0.159 (0.203)	-0.155 (0.187)	-0.196 (0.201)	-0.115 (0.189)	-0.163 (0.203)
Short-run effect	-0.395 (0.270)	-0.471* (0.252)	-0.373 (0.260)	-0.462* (0.247)	-0.402 (0.271)	-0.477* (0.252)	-0.383 (0.262)	-0.471* (0.246)
Medium-run effect	0.018 (0.245)	-0.171 (0.244)	0.142 (0.271)	-0.062 (0.275)	0.003 (0.248)	-0.184 (0.245)	0.123 (0.275)	-0.078 (0.276)
Confounders								
Commodity price shocks			-4.099 (5.139)	-1.838 (4.604)			-4.236 (5.271)	-2.005 (4.657)
Trade liberalization shock					15.581 (14.281)	19.818 (15.971)	19.203 (16.697)	28.521 (23.974)
Year fixed effect	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effect	Y		Y		Y		Y	
Meso-region fixed effect		Y		Y		Y		Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
SE clustered by state	Y		Y		Y		Y	
SE clustered by meso-region		Y		Y		Y		Y

Notes: The dependent variable is measured as 100 times the log change of overall formal employment. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by sectoral exports or imports of each Brazilian trade partner and the sectoral composition of employment at the local labor market. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration in the 1991 Census. The commodity price and trade liberalization shock controls are from Adao [2016] and Dix-Carneiro and Kovak [2017]. Clustered-robust standard-errors in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table B.8: Sectoral employment effects of real exchange rate shocks by different level of sectoral aggregation

	Change in sectoral employment															
	Tradable				Nontradable				Manufacturing				Primary			
	Baseline		2 digits		Baseline		2 digits		Baseline		2 digits		Baseline		2 digits	
	3 digits				3 digits				3 digits				3 digits			
	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)
Export-weighted RER																
Contemporaneous effect	0.342 (0.227)	0.354* (0.209)	0.906*** (0.261)	0.942*** (0.247)	0.106 (0.127)	0.095 (0.123)	-0.304 (0.214)	-0.296 (0.227)	0.753* (0.425)	0.775** (0.388)	0.710* (0.394)	0.717* (0.385)	-0.020 (0.274)	0.006 (0.296)	1.022*** (0.337)	1.067*** (0.376)
Short-run effect	0.506* (0.262)	0.523** (0.216)	0.720*** (0.275)	0.808*** (0.231)	0.174 (0.154)	0.151 (0.123)	0.322** (0.149)	0.363** (0.179)	0.724* (0.376)	0.750** (0.335)	0.837* (0.446)	0.870** (0.407)	0.318 (0.204)	0.369 (0.289)	0.793** (0.387)	0.898*** (0.265)
Medium-run effect	1.044*** (0.353)	1.115*** (0.319)	0.845** (0.395)	0.952*** (0.332)	0.374 (0.252)	0.338 (0.326)	-0.083 (0.302)	-0.058 (0.335)	1.665*** (0.417)	1.759*** (0.411)	1.565** (0.664)	1.546** (0.643)	0.596 (0.417)	0.700 (0.506)	0.806 (0.608)	0.962** (0.442)
Import-weighted RER																
Contemporaneous effect	-0.051 (0.118)	-0.071 (0.096)	0.129 (0.130)	0.073 (0.128)	-0.046 (0.060)	-0.048 (0.075)	-0.207 (0.195)	-0.238 (0.185)	-0.147 (0.242)	-0.151 (0.220)	-0.182 (0.273)	-0.221 (0.259)	-0.151 (0.186)	-0.192 (0.201)	0.021 (0.223)	-0.041 (0.267)
Short-run effect	-0.289* (0.158)	-0.325** (0.133)	-0.111 (0.232)	-0.196 (0.193)	-0.136 (0.096)	-0.140 (0.105)	-0.292 (0.240)	-0.353 (0.219)	-0.473** (0.235)	-0.478** (0.189)	-0.497 (0.518)	-0.558 (0.400)	-0.395 (0.270)	-0.471* (0.252)	-0.017 (0.295)	-0.110 (0.332)
Medium-run effect	0.022 (0.172)	-0.062 (0.158)	-0.190 (0.339)	-0.388 (0.299)	-0.124 (0.094)	-0.140 (0.097)	-0.273 (0.225)	-0.381* (0.224)	-0.444* (0.265)	-0.452** (0.224)	-1.492** (0.637)	-1.603*** (0.479)	0.018 (0.245)	-0.171 (0.244)	0.415 (0.316)	0.178 (0.358)
Year fixed effect	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effect	Y		Y		Y		Y		Y		Y		Y		Y	
Meso-region fixed effect		Y		Y		Y		Y		Y		Y		Y		Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
SE clustered by state	Y		Y		Y		Y		Y		Y		Y		Y	
SE clustered by meso-region		Y		Y		Y		Y		Y		Y		Y		Y

Notes: The dependent variable is measured as 100 times the log change of formal employment in each sector. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by exports or imports of each Brazilian trade partner and the sectoral composition of employment. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration. Clustered-robust standard-errors in parentheses. *** $p < 0.01$, ** $p < 0.5$, * $p < 0.10$.

Table B.9: Sectoral employment effects of real exchange rate shocks by different level of sectoral aggregation

	Change in sectoral employment															
	Tradable				Nontradable				Manufacturing				Primary			
	Baseline		Lagged 1 year		Baseline		Lagged 1 year		Baseline		Lagged 1 year		Baseline		Lagged 1 year	
	3-year moving avg.				3-year moving avg.				3-year moving avg.				3-year moving avg.			
	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)
Export-weighted RER																
Contemporaneous effect	0.342 (0.227)	0.354* (0.209)	0.438 (0.278)	0.437** (0.218)	0.106 (0.127)	0.095 (0.123)	-0.111 (0.220)	-0.123 (0.181)	0.753* (0.425)	0.775** (0.388)	0.910 (0.576)	0.892* (0.501)	-0.020 (0.274)	0.006 (0.296)	-0.079 (0.246)	-0.055 (0.283)
Short-run effect	0.506* (0.262)	0.523** (0.216)	0.461 (0.301)	0.468* (0.243)	0.174 (0.154)	0.151 (0.123)	0.054 (0.260)	0.029 (0.265)	0.724* (0.376)	0.750** (0.335)	0.822 (0.562)	0.806* (0.441)	0.318 (0.204)	0.369 (0.289)	0.350 (0.306)	0.395 (0.272)
Medium-run effect	1.044*** (0.353)	1.115*** (0.319)	1.256*** (0.407)	1.314*** (0.358)	0.374 (0.252)	0.338 (0.326)	0.126 (0.144)	0.076 (0.212)	1.665*** (0.417)	1.759*** (0.411)	2.309*** (0.639)	2.364*** (0.563)	0.596 (0.417)	0.700 (0.506)	0.457 (0.489)	0.520 (0.548)
Import-weighted RER																
Contemporaneous effect	-0.051 (0.118)	-0.071 (0.096)	-0.067 (0.111)	-0.083 (0.088)	-0.046 (0.060)	-0.048 (0.075)	-0.043 (0.048)	-0.034 (0.063)	-0.147 (0.242)	-0.151 (0.220)	-0.252 (0.193)	-0.262 (0.194)	-0.151 (0.186)	-0.192 (0.201)	-0.074 (0.200)	-0.094 (0.199)
Short-run effect	-0.289* (0.158)	-0.325** (0.133)	-0.327** (0.164)	-0.359*** (0.124)	-0.136 (0.096)	-0.140 (0.105)	-0.208*** (0.077)	-0.191* (0.100)	-0.473** (0.235)	-0.478** (0.189)	-0.607*** (0.205)	-0.630*** (0.162)	-0.395 (0.270)	-0.471* (0.252)	-0.292 (0.246)	-0.330 (0.232)
Medium-run effect	0.022 (0.172)	-0.062 (0.158)	-0.067 (0.186)	-0.149 (0.164)	-0.124 (0.094)	-0.140 (0.097)	-0.101 (0.093)	-0.073 (0.102)	-0.444* (0.265)	-0.452** (0.224)	-0.513** (0.253)	-0.584** (0.238)	0.018 (0.245)	-0.171 (0.244)	-0.076 (0.294)	-0.168 (0.269)
Year fixed effect	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effect	Y		Y		Y		Y		Y		Y		Y		Y	
Meso-region fixed effect		Y		Y		Y		Y		Y		Y		Y		Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
SE clustered by state	Y		Y		Y		Y		Y		Y		Y		Y	
SE clustered by meso-region		Y		Y		Y		Y		Y		Y		Y		Y

Notes: The dependent variable is measured as 100 times the log change of formal employment in each sector. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by exports or imports of each Brazilian trade partner and the sectoral composition of employment. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration. Clustered-robust standard-errors in parentheses. *** $p < 0.01$, ** $p < 0.5$, * $p < 0.10$.

Table B.10: Sectoral employment effects of real exchange rate shocks by different level of sectoral aggregation

	Change in sectoral employment															
	Tradable				Nontradable				Manufacturing				Primary			
	Baseline		Lagged 2 years		Baseline		Lagged 2 years		Baseline		Lagged 2 years		Baseline		Lagged 2 years	
	3-year moving avg.				3-year moving avg.				3-year moving avg.				3-year moving avg.			
	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)	(3)	(5)
Export-weighted RER																
Contemporaneous effect	0.342 (0.227)	0.354* (0.209)	-0.025 (0.183)	-0.024 (0.238)	0.106 (0.127)	0.095 (0.123)	0.145 (0.174)	0.143 (0.169)	0.753* (0.425)	0.775** (0.388)	-0.023 (0.271)	-0.018 (0.380)	-0.020 (0.274)	0.006 (0.296)	0.296 (0.232)	0.321 (0.237)
Short-run effect	0.506* (0.262)	0.523** (0.216)	0.542** (0.223)	0.550** (0.240)	0.174 (0.154)	0.151 (0.123)	0.083 (0.224)	0.078 (0.215)	0.724* (0.376)	0.750** (0.335)	0.665 (0.412)	0.692 (0.464)	0.318 (0.204)	0.369 (0.289)	0.359 (0.437)	0.412 (0.413)
Medium-run effect	1.044*** (0.353)	1.115*** (0.319)	0.740** (0.334)	0.801** (0.329)	0.374 (0.252)	0.338 (0.326)	0.571 (0.386)	0.563 (0.367)	1.665*** (0.417)	1.759*** (0.411)	1.422*** (0.373)	1.561*** (0.509)	0.596 (0.417)	0.700 (0.506)	0.754 (0.502)	0.847 (0.516)
Import-weighted RER																
Contemporaneous effect	-0.051 (0.118)	-0.071 (0.096)	-0.252*** (0.086)	-0.272*** (0.076)	-0.046 (0.060)	-0.048 (0.075)	-0.172** (0.075)	-0.172*** (0.064)	-0.147 (0.242)	-0.151 (0.220)	-0.357** (0.146)	-0.373** (0.148)	-0.151 (0.186)	-0.192 (0.201)	-0.196 (0.161)	-0.222* (0.125)
Short-run effect	-0.289* (0.158)	-0.325** (0.133)	-0.299*** (0.108)	-0.342*** (0.098)	-0.136 (0.096)	-0.140 (0.105)	-0.179** (0.090)	-0.179** (0.074)	-0.473** (0.235)	-0.478** (0.189)	-0.364** (0.155)	-0.397** (0.164)	-0.395 (0.270)	-0.471* (0.252)	-0.320* (0.192)	-0.376** (0.168)
Medium-run effect	0.022 (0.172)	-0.062 (0.158)	-0.065 (0.161)	-0.172 (0.151)	-0.124 (0.094)	-0.140 (0.097)	-0.151 (0.112)	-0.155 (0.112)	-0.444* (0.265)	-0.452** (0.224)	-0.283 (0.220)	-0.361 (0.233)	0.018 (0.245)	-0.171 (0.244)	-0.019 (0.213)	-0.132 (0.202)
Year fixed effect	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effect	Y		Y		Y		Y		Y		Y		Y		Y	
Meso-region fixed effect		Y		Y		Y		Y		Y		Y		Y		Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
SE clustered by state	Y		Y		Y		Y		Y		Y		Y		Y	
SE clustered by meso-region		Y		Y		Y		Y		Y		Y		Y		Y

Notes: The dependent variable is measured as 100 times the log change of formal employment in each sector. The measures of the real exchange rate shock at the local level are computed as a shift-share variable, where bilateral real exchange rate shocks are weighted by exports or imports of each Brazilian trade partner and the sectoral composition of employment. Controls include income per capita, the share of workforce in rural areas, in the informal sector and in the public administration. Clustered-robust standard-errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

APPENDIX C

ADDITIONAL RESULTS OF CHAPTER 3

Partisanship classification

This appendix provides information on how we assign party ideology. To calculate the left margin of victory, we classify all parties that participated in the municipal elections of 2004, 2008 and 2012 as left or non-left (centrist, right or neither). This is not an easy task given that Brazil has one of the most fragmented party system in the world [Cesar Zucco and Power, 2021], with 33 registered parties in Brazil’s Electoral Court in 2018. Therefore, we base our classification using previous literature and only assign party ideology based on other sources for a few cases. In what follows we detail how candidate’s partisanship was coded.

First, we use Cesar Zucco and Power [2021], Zucco and Power [2012, 2009]’s classification as our main source of party ideology. The classification is based on eight waves of the Brazilian Legislative Surveys (BLS) that have been carried out by the authors since the redemocratization of the country [Timothy and Cesar, 2011]. The survey asks each legislator questions that require them to position themselves and all main parties in the political system on a “left-right” scale. Based on these answers, the authors create scores for each party in the “left-right” scale, where all parties to the left of PV (PV inclusive) in the 2017 survey is classified as left and to the right as non-left [Cesar Zucco and Power, 2021, p. 5]. We classify 15 parties in this way.¹

Second, we use Baker and Greene [2011] partisanship codes to classify other 12 parties. Baker and Greene [2011] provides scores in the left-right scale for all parties in Latin America that participated in a presidential election between 1995 and 2008.

Third, we follow Girardi [2020] and assign partisanship based on party international partisan association for all other cases. All parties affiliated to the Socialist International, Foro de São Paulo, Party of European Socialists or Progressive Alliance are coded as left. All the remaining parties are classified as non-left (centrist, right or neither).

¹Even though party scores change for every survey-year, none of the parties switch from right to left (or the other way around) of PV score for the year 2017.

Table C.1 reports the final classification with the respective source from which the party ideology was assigned.

Table C.1: Party classification

Leftist parties		Non-leftist parties	
Party	Source	Party	Source
PV	Zucco and Power (2018)	DEM/PFL	Zucco and Power (2018)
PT	Zucco and Power (2018)	MDB/PMDB	Zucco and Power (2018)
PSOL	Zucco and Power (2018)	PP	Zucco and Power (2018)
PSB	Zucco and Power (2018)	PR	Zucco and Power (2018)
PPS/CID	Zucco and Power (2018)	PRB	Zucco and Power (2018)
PDT	Zucco and Power (2018)	PSDB	Zucco and Power (2018)
PCdoB	Zucco and Power (2018)	PSL	Zucco and Power (2018)
PPL	Foro de São Paulo	PTB	Zucco and Power (2018)
PSTU	Baker and Greene (2011)	PRONA	Baker and Greene (2011)
PMN	Baker and Greene (2011)	PRP	Baker and Greene (2011)
PCO	Baker and Greene (2011)	PRTB	Baker and Greene (2011)
PCB	Baker and Greene (2011)	PSC	Baker and Greene (2011)
		DC/PSDC	Baker and Greene (2011)
		PODE/PTN	Baker and Greene (2011)
		PTdoB	
		PAN	
		PHS	
		PL	
		PSD	
		PTC	
		PEN	

Leftist parties: Partido Verde (PV), Partido dos Trabalhadores (PT), Partido Socialismo e Liberdade (PSOL), Partido Socialista Brasileiro (PSB), Partido Popular Socialista/Cidadania (PPS/CID), Partido Democrático Trabalhista (PDT), Partido Comunista do Brasil (PCdoB), Partido Pátria Livre (PPL), Partido Socialista dos Trabalhadores Unificado (PSTU), Partido da Mobilização Nacional (PMN), Partido da Causa Operária (PCO), Partido Comunista Brasileiro (PCB).

Non-leftist parties: Democratas/Partido da Frente Liberal (DEM/PFL), Movimento Democrático Brasileiro/Partido do Movimento Democrático Brasileiro (MDB/PMDB), Partido Progressista (PP), Partido da República (PR), Partido Republicano Brasileiro (PRB), Partido da Social Democracia Brasileira (PSDB), Partido Social Liberal (PSL), Partido Trabalhista Brasileiro (PTB), Partido da Reedificação da Ordem Nacional (PRONA), Partido Republicano Progressista (PRP), Partido Renovador Trabalhista Brasileiro (PRTB), Partido Social Cristão (PSC), Democracia Cristã/Partido Social Democrata Cristão (DC/PSDC), Podemos/Partido Trabalhista Nacional (PODE/PTN), Partido Trabalhista do Brasil (PTdoB), Partido dos Aposentados da Nação (PAN), Partido Humanista da Solidariedade (PHS), Partido Liberal (PL), Partido Social Democrático (PSD), Partido Trabalhista Cristão (PTC), Partido Ecológico Nacional (PEN).

Covariates descriptive statistics

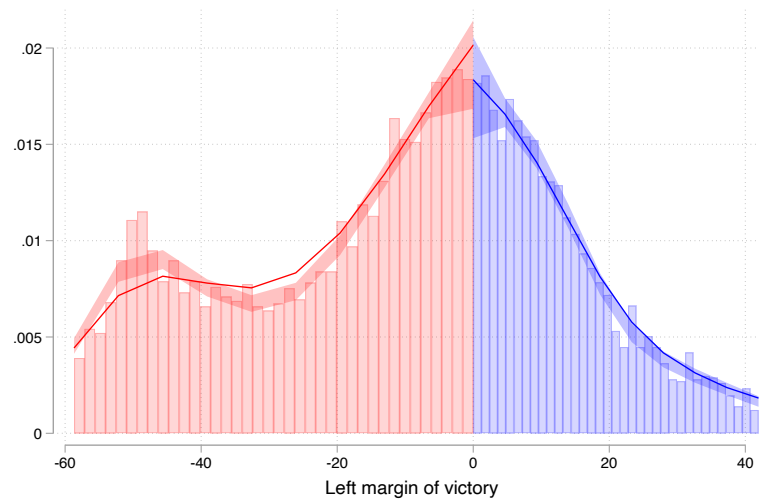
Table C.2: Covariates descriptive statistics

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Labor market and demographic covariates					
Median earnings	806.89 (269.84)	809.02 (286.18)	873.48 (294.44)	822.26 (280.56)	786.95 (305.44)
Labor force participation	54.87 (8.43)	54.78 (8.79)	55.37 (7.59)	55.02 (8.45)	54.07 (6.93)
Population (in thousands)	43.24 (253.47)	46.50 (217.32)	65.47 (342.65)	58.98 (373.28)	67.47 (247.91)
% Urban	62.55 (23.82)	62.73 (24.25)	67.39 (23.67)	63.85 (24.08)	65.72 (24.84)
% White	50.93 (25.03)	51.62 (25.05)	52.07 (24.37)	50.42 (25.34)	42.31 (21.87)
% Higher education	3.69 (3.19)	3.61 (3.15)	4.22 (3.55)	3.89 (3.32)	3.42 (3.67)
% Illiterate	17.59 (10.98)	18.09 (11.53)	15.38 (10.45)	17.26 (10.97)	19.34 (11.31)
Geographic covariates					
North	0.08 (0.27)	0.07 (0.26)	0.11 (0.32)	0.08 (0.27)	0.03 (0.16)
Northeast	0.33 (0.47)	0.34 (0.48)	0.24 (0.43)	0.35 (0.48)	0.51 (0.50)
South	0.21 (0.41)	0.22 (0.41)	0.19 (0.39)	0.23 (0.42)	0.06 (0.24)
Southeast	0.32 (0.47)	0.31 (0.46)	0.40 (0.49)	0.28 (0.45)	0.41 (0.49)
Midwest	0.07 (0.25)	0.06 (0.23)	0.06 (0.24)	0.05 (0.22)	0.00 (0.00)
Other covariates					
Bolsa Familia (households)	2.16 (7.00)	2.37 (7.38)	2.89 (9.66)	2.69 (8.98)	2.98 (7.45)
Bolsa Familia (receipts)	142.23 (113.04)	145.52 (112.82)	123.94 (102.01)	148.70 (114.60)	133.97 (91.66)
Authorized amendments	1.56 (6.65)	1.60 (6.08)	1.82 (7.68)	1.75 (7.85)	1.26 (2.33)
Executed amendments	0.64 (4.90)	0.80 (4.66)	0.76 (5.88)	0.74 (6.34)	0.66 (1.76)
Number of obs.	8943	2395	4158	3105	919

Notes: This table reports mean and standard deviation (in parenthesis) for the covariate variables. Demographic and geographic covariates obtained from IBGE. Data on the conditional cash-transfer program Bolsa Família are from *Ministério da Cidadania*. Both the number of households receiving Bolsa Família and Bolsa Família receipts are normalized by population to take into account city size. Transfers received through amendments are expressed as a share of city revenues and obtained from *SIGA-Brasil*. See Section 3.6 for the specific definition and motivation of each subsample.

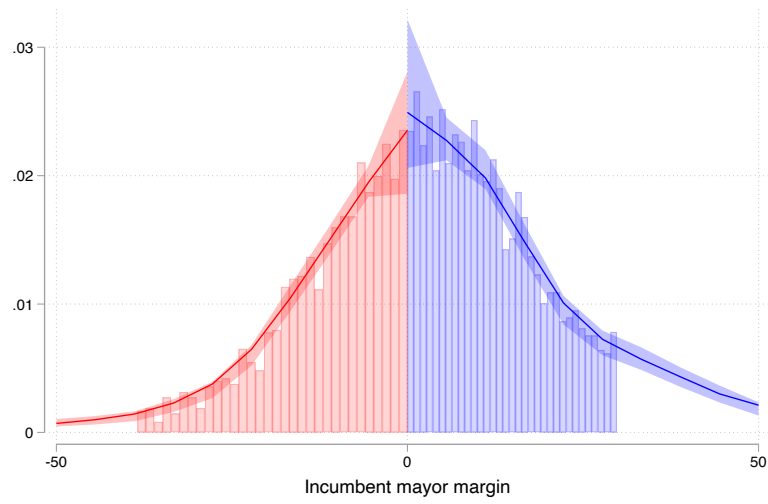
Additional design assessment tests

Figure C.1: Test for manipulation of the running variable



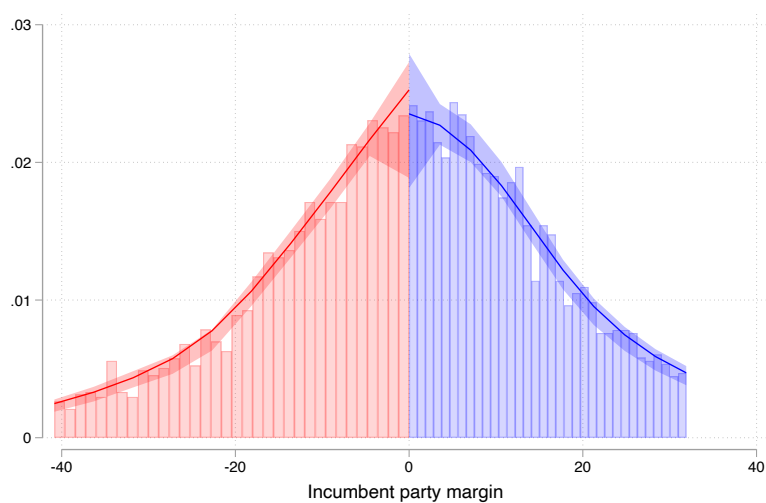
Notes: The figure presents visual evidence for the Cattaneo et al. [2018] manipulation test. The null hypothesis is that there is no discontinuity in the distribution of the running variable at the cutoff. T-stat = -0.65; P-value = 0.51.

Figure C.2: Test for manipulation by incumbent mayors



Notes: The figure presents visual evidence for the Cattaneo et al. [2018] manipulation test. The null hypothesis is that there is no discontinuity in the distribution of the incumbent margin at the cutoff. This test focuses on elections in which one of the candidates is the incumbent mayor. T-stat = 1.16; P-value = 0.25.

Figure C.3: Test for manipulation by incumbent parties



Notes: The figure presents visual evidence for the Cattaneo et al. [2018] manipulation test. The null hypothesis is that there is no discontinuity in the distribution of the incumbent margin at the cutoff. This test focuses on elections in which one of the candidates is affiliated with the party of the incumbent mayor (either the incumbent herself or a different candidate from the same party). T-stat = 0.50; P-value = 0.62.

Tests for balance in candidate characteristics

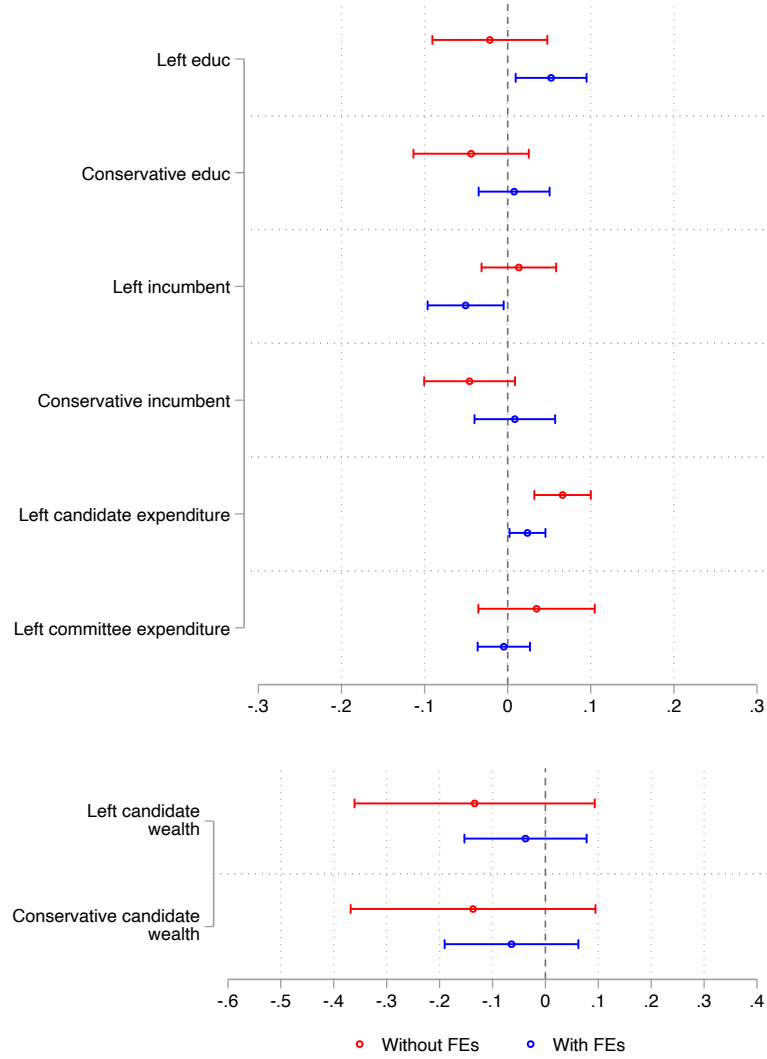
This appendix presents tests for discontinuities in candidates' characteristics around the threshold, in the spirit of Caughey and Sekhon [2011]. In particular, we look at candidates' education level, incumbency status, self-reported personal wealth, and their campaign expenditures. For campaign expenditures, we look at both expenditures made by the individual candidate, and expenditures made by the party election committee on the candidate's behalf.²

Specifically, we estimate our RD specification (equation 3.2), again with the margin of the left-wing candidate as the running variable, but with the characteristics of both left and conservative candidates as the outcomes. In other words, this test compares close left-wing winners to close left-wing losers (and does the same for conservative candidates).

Results are reported in Figure C.4, for the baseline sample, both with and without controlling for city and time fixed effects. Overall, we find little evidence of discontinuities in candidates' education level, personal wealth and incumbency status. When not including city and time fixed effects, we do find some significant discontinuity in candidate expenditures: close winners tend to have spent more during the campaign than close losers. This is similar to what Caughey and Sekhon [2011] found in a sample of US House elections. However, this discontinuity in campaign expenditures becomes much smaller once we control for city and time fixed effects.

²Incumbency status is measured by a dummy variable equal to one if the candidate is the incumbent mayor; education level by a dummy equal to one if the candidate completed high school; campaign expenditures are normalized by the total expenditure of the two relevant candidates, therefore the relevant variable is the share of candidate expenditures made the left-wing candidate.

Figure C.4: Discontinuities in candidate characteristics around the threshold



Notes: The figure presents estimates from our RD specification (equation 3.2), with candidates' characteristics as outcome variables. 'Left educ' is a dummy for whether the left candidate has completed high school. 'Left incumbent' is a dummy for whether the left candidate is the incumbent mayor. Candidate and committee expenditures are measured as a share of the total expenditures of the two relevant candidates. Wealth is the log of self-reported wealth.

Composition of revenues

Table C.3: RD estimates of the effect of a left-wing mayor on the composition of revenues

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Revenues, % GDP					
Total revenue	0.12 (0.25)	-1.14 (1.20)	-0.20 (0.42)	0.24 (0.27)	-0.21 (0.49)
of which:					
Municipal taxes	-0.02 (0.03)	-0.07 (0.13)	-0.10 (0.05)	0.05 (0.05)	-0.02 (0.06)
Federal transfers	0.15 (0.13)	-0.44 (0.56)	0.05 (0.22)	0.09 (0.14)	-0.12 (0.32)
State transfers	0.02 (0.06)	-0.18 (0.23)	-0.07 (0.11)	0.05 (0.08)	-0.01 (0.15)
Composition of revenues: revenue categories (% of total revenues)					
Municipal taxes	-0.17 (0.09)	0.13 (0.22)	-0.21 (0.15)	0.01 (0.15)	0.20 (0.26)
Federal transfers	0.30 (0.19)	0.37 (0.44)	0.66 (0.28)	-0.22 (0.33)	0.36 (0.78)
State transfers	-0.03 (0.14)	-0.04 (0.29)	0.03 (0.23)	0.28 (0.27)	0.13 (0.52)
Observations (all)	8943	2395	4158	3105	919
Observations (effective)	5168	1786	3177	1679	366

Notes: Estimates from our baseline RD specification (equation 3.2), using the bias-corrected procedure of Calonico et al. [2014] and controlling for city and year fixed effects. Outcomes measured as 4-year averages over a mayoral term. Per-capita variables are taken in logarithms and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Dynamic effects and pre-trends

Table C.4: Dynamic effects and pre-trends in the baseline sample

Outcome	Previous Mandate	Overall Mandate	Dynamics			
	Average	Average	1st year	2nd year	3rd year	4th year
Size of government: overall revenues and expenses						
Expenditure per capita	-0.20 (0.52)	-0.01 (0.56)	1.05 (0.89)	0.07 (0.80)	0.15 (0.65)	-0.41 (0.79)
Expenditure, % of GDP	0.40 (0.26)	0.01 (0.24)	0.35 (0.17)	0.14 (0.19)	-0.51 (0.81)	0.00 (0.18)
Revenue per capita	-0.58 (0.55)	0.39 (0.52)	-0.01 (0.64)	1.38 (0.90)	-0.17 (0.68)	0.50 (0.89)
Revenue, % of GDP	0.20 (0.25)	0.12 (0.25)	0.34 (0.19)	0.29 (0.20)	-0.33 (0.75)	0.35 (0.22)
Allocation of resources: budget categories (% of total expenditure)						
Current Expenditure	-0.01 (0.24)	-0.05 (0.17)	0.10 (0.26)	0.20 (0.30)	-0.27 (0.27)	-0.33 (0.30)
of which:						
Personnel	-0.19 (0.25)	-0.05 (0.22)	-0.28 (0.34)	0.30 (0.27)	-0.02 (0.27)	-0.23 (0.29)
Public Investment	-0.01 (0.24)	0.09 (0.16)	-0.08 (0.26)	-0.17 (0.30)	0.37 (0.27)	0.27 (0.29)
Allocation of resources: functional categories (% of total expenditure)						
Social Expenditures	-0.13 (0.21)	0.64 (0.21)	0.13 (0.30)	0.78 (0.30)	0.66 (0.32)	1.05 (0.34)
of which:						
Health & sanitation	-0.16 (0.16)	0.18 (0.15)	-0.23 (0.20)	0.12 (0.21)	0.25 (0.22)	0.58 (0.21)
Education & culture	0.08 (0.16)	0.24 (0.16)	0.32 (0.22)	0.47 (0.23)	0.09 (0.22)	0.35 (0.24)
Social welfare	-0.07 (0.06)	0.16 (0.06)	0.09 (0.08)	0.19 (0.08)	0.18 (0.08)	0.13 (0.07)
Other expenditures:						
Housing	-0.00 (0.15)	-0.16 (0.13)	-0.18 (0.21)	-0.18 (0.20)	-0.12 (0.18)	-0.17 (0.20)
Transportation	-0.24 (0.13)	-0.18 (0.09)	-0.16 (0.13)	-0.30 (0.15)	-0.25 (0.13)	-0.15 (0.14)
Other	0.36 (0.24)	-0.21 (0.23)	0.16 (0.30)	-0.32 (0.32)	-0.11 (0.33)	-0.81 (0.40)
Social Expenditures per capita	-0.23 (0.65)	1.16 (0.61)	1.23 (0.98)	2.42 (1.10)	0.96 (0.81)	1.01 (0.92)
Observations (all)	8144	8943	8943	8943	8943	8943
Observations (effective)	5070	4408	3160	4565	5385	4166

Notes: Estimation of equation 3.2, using the Calonico et al. [2014] procedure and controlling for city and year fixed effects. Outcomes are 4-year averages over a mayoral term or the outcome of an individual year of the mandate. Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Table C.5: Dynamic effects and pre-trends in the lameduck subsample

Outcome	Previous Mandate	Overall Mandate	Dynamics			
	Average	Average	1st year	2nd year	3rd year	4th year
Size of government: overall revenues and expenses						
Expenditure per capita	-1.58 (1.09)	0.52 (1.27)	3.59 (1.97)	1.40 (1.81)	-0.43 (1.57)	-0.50 (1.64)
Expenditure, % of GDP	0.45 (0.65)	-1.05 (1.18)	0.63 (0.36)	0.15 (0.35)	-4.45 (4.58)	0.37 (0.39)
Revenue per capita	-0.55 (1.04)	0.59 (1.21)	0.54 (1.63)	2.28 (1.83)	0.13 (1.42)	0.09 (1.51)
Revenue, % of GDP	0.59 (0.67)	-1.14 (1.20)	0.11 (0.39)	0.23 (0.38)	-4.59 (4.68)	0.31 (0.40)
Allocation of resources: budget categories (% of total expenditure)						
Current Expenditure	0.38 (0.42)	-0.39 (0.38)	-0.35 (0.56)	-0.32 (0.59)	-0.61 (0.55)	-0.32 (0.55)
of which:						
Personnel	0.49 (0.52)	-0.81 (0.48)	-1.14 (0.66)	-0.20 (0.67)	-0.93 (0.62)	-1.23 (0.61)
Public Investment	-0.28 (0.43)	0.40 (0.37)	0.49 (0.56)	0.16 (0.66)	0.51 (0.54)	0.27 (0.59)
Allocation of resources: functional categories (% of total expenditure)						
Social Expenditures	-0.25 (0.39)	1.27 (0.40)	1.00 (0.57)	1.52 (0.56)	0.77 (0.63)	2.19 (0.85)
of which:						
Health & sanitation	-0.13 (0.29)	0.69 (0.30)	0.28 (0.39)	0.69 (0.45)	0.55 (0.48)	1.12 (0.45)
Education & culture	-0.06 (0.30)	0.31 (0.30)	0.27 (0.48)	0.57 (0.42)	0.18 (0.43)	0.97 (0.63)
Social welfare	-0.07 (0.13)	0.32 (0.11)	0.43 (0.15)	0.38 (0.17)	0.19 (0.14)	0.21 (0.12)
Other expenditures:						
Housing	-0.03 (0.35)	-0.36 (0.29)	-0.30 (0.46)	-0.52 (0.42)	-0.61 (0.42)	-0.05 (0.43)
Transportation	-0.69 (0.25)	-0.07 (0.20)	0.07 (0.28)	0.25 (0.31)	0.06 (0.28)	-0.30 (0.34)
Other	0.84 (0.44)	-0.86 (0.42)	-0.70 (0.57)	-1.26 (0.56)	-0.16 (0.70)	-2.00 (0.87)
Social Expenditures per capita	-1.89 (1.36)	3.34 (1.36)	4.45 (2.04)	1.94 (2.67)	0.76 (2.52)	-0.66 (3.16)
Observations (all)	2227	2395	2395	2395	2395	2395
Observations (effective)	1185	1227	857	1150	1266	1146

Notes: Estimation of equation 3.2, using the Calonico et al. [2014] procedure and controlling for city and year fixed effects. Outcomes are 4-year averages over a mayoral term or the outcome of an individual year of the mandate. Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Table C.6: Dynamic effects and pre-trends in the oil windfall subsample

Outcome	Previous Mandate	Overall Mandate	Dynamics			
	Average	Average	1st year	2nd year	3rd year	4th year
Size of government: overall revenues and expenses						
Expenditure per capita	0.20 (2.20)	2.26 (2.06)	2.45 (2.46)	0.68 (2.44)	3.86 (2.76)	2.06 (2.82)
Expenditure, % of GDP	-0.02 (0.40)	-0.13 (0.47)	0.85 (0.58)	-0.30 (0.62)	-0.37 (0.62)	-0.60 (0.75)
Revenue per capita	1.52 (2.64)	2.19 (2.01)	2.79 (2.63)	-0.04 (2.75)	4.11 (2.24)	-0.76 (2.60)
Revenue, % of GDP	0.05 (0.47)	-0.21 (0.49)	0.83 (0.61)	-0.57 (0.58)	-0.31 (0.65)	-0.87 (0.83)
Allocation of resources: budget categories (% of total expenditure)						
Current Expenditure	0.49 (0.81)	1.01 (0.65)	0.79 (0.83)	1.36 (1.07)	-0.27 (0.93)	2.67 (1.02)
of which:						
Personnel	-0.35 (0.72)	-0.34 (0.85)	-0.29 (1.09)	0.37 (1.15)	-1.10 (1.08)	0.39 (1.09)
Public Investment	-0.13 (0.80)	-0.96 (0.63)	-0.57 (0.84)	-1.19 (1.06)	0.05 (0.92)	-2.67 (1.03)
Allocation of resources: functional categories (% of total expenditure)						
Social Expenditures	-0.34 (0.73)	2.19 (0.87)	2.09 (1.26)	2.34 (1.15)	1.05 (1.06)	3.92 (1.85)
of which:						
Health & sanitation	-0.92 (0.55)	0.45 (0.42)	0.95 (0.97)	1.00 (0.68)	0.42 (0.64)	1.16 (0.75)
Education & culture	0.58 (0.59)	0.83 (0.50)	0.75 (0.81)	0.74 (0.73)	0.29 (0.71)	2.34 (1.17)
Social welfare	0.08 (0.24)	0.36 (0.20)	0.39 (0.24)	0.33 (0.30)	0.36 (0.27)	0.33 (0.28)
Other expenditures:						
Housing	0.45 (0.64)	0.41 (0.65)	0.45 (0.80)	0.46 (0.97)	0.03 (0.78)	0.45 (0.92)
Transportation	-0.26 (0.28)	-0.73 (0.29)	-0.49 (0.34)	-0.59 (0.37)	-0.63 (0.35)	-0.87 (0.38)
Other	0.15 (0.80)	-1.90 (1.10)	-1.79 (1.60)	-2.09 (1.39)	-0.29 (1.22)	-3.53 (2.19)
Social Expenditures per capita	0.08 (2.42)	6.48 (2.40)	7.60 (3.43)	3.70 (2.73)	4.56 (2.80)	3.89 (2.74)
Observations (all)	813	919	919	919	919	919
Observations (effective)	487	451	386	515	482	417

Notes: Estimation of equation 3.2, using the Calonico et al. [2014] procedure and controlling for city and year fixed effects. Outcomes are 4-year averages over a mayoral term or the outcome of an individual year of the mandate. Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Results by mayoral term and extended sample period

This section shows how the effect of a left-wing mayor on the share of social spending has varied over time. In this exercise, we extend our sample period backwards to the 1996 and 2000 electoral cycles, in order to provide a broader picture of how partisan effects evolved in time after Brazil’s democratization. Extending our sample period backwards is challenging for two reasons, however, and results using pre-2004 data should be taken with caution.

First, TSE informs in the website of its main statistical repository that electoral data for pre-2004 elections are incomplete. Indeed, the raw files have data only on 116 cities for the 1996 election. However, data on the 2000 election seem complete as they contain information on 5,555 cities. For the 1996 election, we solve the problem by downloading data from an old repository from TSE. Even though this old repository has all the key variables for our study, it does not contain information about the status of the election (whether or not the election was judged irregular and redone outside the regular calendar) or the final status of the candidates registration (whether a candidate died, renounced or had any other irregularities), both of which we use in our sample selection procedure. Given that the data from the main repository seem complete for the 2000 elections and the fact that the old repository has important limitations, we decided to use data from the former for the 2000 election.

The second problem is that pre- and post-2002 fiscal data are not fully comparable. In particular, besides other issues, social spending cannot be calculated properly in the pre-2002 period, because welfare spending (which is part of our social spending variable) cannot be separated from pensions paid to former municipal employees (which are not). This second problem cannot be completely solved, which is the main reason why we don’t include pre-2004 elections in our main analysis. Here, we adopt a second-best solution. We compute an alternative definition of social spending that can be calculated both pre- and post-2002. This measure is equal to our preferred definition of social spending, plus pensions paid to former municipal employees.

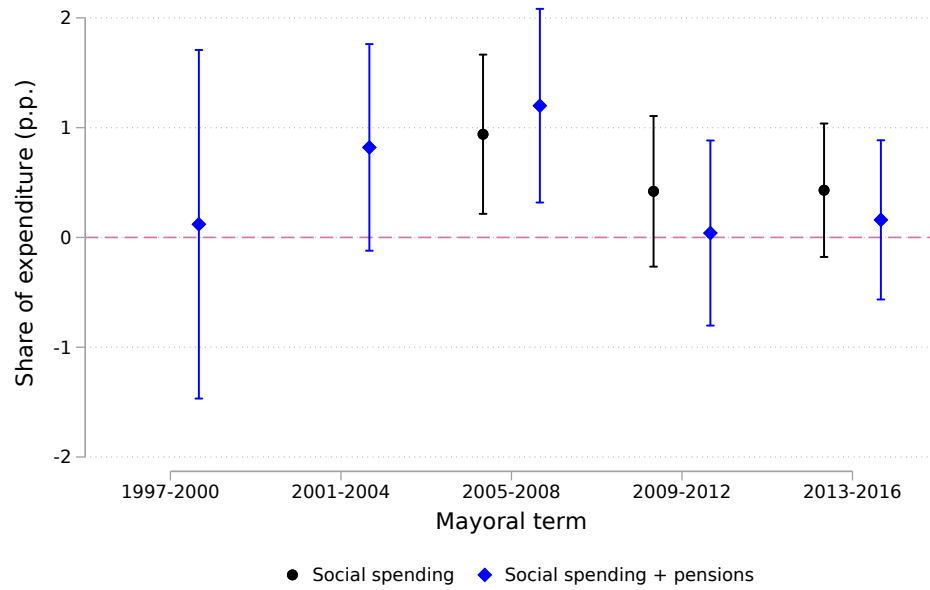
Figure C.5 displays the estimated effect of a left-wing mayor on the share of social spending by election cycle. We use both the proper definition of social spending (which excludes pensions to former municipal employees and is available only post-2002) and the ‘second-best’ one (which includes pensions and is available for all elections).

The effect on the share of social spending is positive in all periods and with both definitions, but it seems much stronger in the ‘boom years’ 2001-2008, and weaker pre-

2001 and post-2008. The effect seems virtually null but very imprecisely estimated in the 1997-2000 period (that is, for mayors elected in 1996), but the large standard errors and the unavailability of the proper definition of social spending for that period suggest much caution in interpreting that result.

The fact that the effect is strongest in the 2001-2008 ‘boom years’ appears consistent with the hypothesis that left-wing mayors redistribute more when financial constraints are relaxed: the boom years were characterized by rising revenues, caused both by strong income growth and the commodities boom. There does not appear to be any clear mapping between the intensity of partisan effects and the balance of power at the federal level. The left-wing PT held the presidency in the 2002-2008 period, in which partisan effects are stronger, but also in 2009-2016 (except for the second half of 2016), when the effect was weaker.

Figure C.5: Effect on the share of social expenditures, by mayoral term



Notes: Effect of a left-wing mayor on the share of social spending from our RD specification (equation 3.2), using the robust and bias-corrected procedure of Calonico et al. [2014] and controlling for city and year fixed effects. The effect is estimated separately for each mayoral term, using our baseline definition of social spending (black) and an alternative definition that includes pensions paid to former municipal employees (blue). Bars represent 95% confidence intervals from robust bias-corrected standard errors clustered by municipality.

Table C.7: RD estimates of the effect of a left-wing mayor, using differenced outcomes

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Size of government: overall revenues and expenses					
Expenditure per capita	0.22 (1.45)	0.61 (2.62)	2.30 (2.23)	-3.02 (2.18)	0.86 (4.13)
Expenditure, % of GDP	-0.03 (0.36)	0.50 (0.64)	-0.11 (0.50)	-0.97 (0.58)	-0.88 (1.10)
Revenue per capita	1.40 (1.32)	0.36 (2.04)	0.13 (1.96)	0.79 (2.00)	-5.20 (4.90)
Revenue, % of GDP	0.29 (0.35)	0.25 (0.68)	-0.35 (0.52)	-0.27 (0.57)	-1.88 (1.29)
Allocation of resources: budget categories (% of total expenditure)					
Current Expenditure	-0.30 (0.58)	-0.87 (0.94)	0.13 (0.80)	0.00 (0.88)	4.10 (2.02)
of which:					
Personnel	0.39 (0.54)	-1.02 (0.96)	-0.00 (0.77)	0.64 (0.90)	3.59 (1.88)
Public Investment	0.53 (0.60)	0.93 (0.99)	0.32 (0.81)	0.54 (0.87)	-3.69 (2.04)
Allocation of resources: functional categories (% of total expenditure)					
Social Expenditures	1.31 (0.61)	2.44 (1.48)	2.54 (0.84)	2.76 (1.07)	3.14 (2.64)
of which:					
Health & sanitation	1.28 (0.41)	0.98 (0.80)	1.77 (0.54)	1.53 (0.67)	2.61 (1.35)
Education & culture	-0.07 (0.39)	1.96 (1.09)	0.43 (0.59)	1.03 (0.66)	0.81 (1.79)
Social welfare	0.24 (0.13)	-0.05 (0.25)	0.38 (0.18)	0.27 (0.22)	-0.22 (0.71)
Other expenditures:					
Housing	0.08 (0.40)	0.72 (0.86)	0.78 (0.62)	1.19 (0.67)	-0.67 (1.65)
Transportation	0.09 (0.28)	0.13 (0.69)	-0.56 (0.38)	-0.48 (0.44)	0.09 (0.57)
Other	-1.57 (0.72)	-3.28 (1.56)	-2.83 (1.04)	-3.59 (1.27)	-2.88 (3.05)
Social Expenditures per capita	1.56 (1.87)	-1.42 (5.11)	4.70 (2.22)	-1.45 (2.79)	0.98 (4.91)
Observations (all)	8502	2320	3969	2963	859
Observations (effective)	3881	1150	1919	1585	460

Notes: Estimates from our baseline RD specification (equation 3.2), using the bias-corrected procedure of Calonico et al. [2014] and controlling for year fixed effects. All outcomes taken as percentage points differences between the fourth year of the term and the election year. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Table C.8: RD estimates of the effect of a left-wing mayor - excluding first year of mayor term

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Size of government: overall revenues and expenses					
Expenditure per capita	-0.24 (0.56)	0.22 (1.31)	-0.60 (0.82)	-0.89 (1.33)	2.22 (2.22)
Expenditure, % of GDP	-0.11 (0.31)	-1.40 (1.55)	-0.47 (0.58)	-1.37 (1.18)	-0.42 (0.54)
Revenue per capita	0.36 (0.55)	0.42 (1.25)	-0.44 (0.82)	-0.12 (1.30)	1.88 (2.13)
Revenue, % of GDP	0.06 (0.32)	-1.45 (1.58)	-0.32 (0.58)	-1.21 (1.18)	-0.49 (0.55)
Allocation of resources: budget categories (% of total expenditure)					
Current Expenditure	-0.13 (0.18)	-0.37 (0.41)	-0.24 (0.29)	0.31 (0.48)	1.06 (0.73)
of which:					
Personnel	0.00 (0.22)	-0.67 (0.51)	0.05 (0.33)	0.28 (0.46)	-0.27 (0.91)
Public Investment	0.20 (0.19)	0.34 (0.41)	0.31 (0.29)	-0.07 (0.42)	-1.04 (0.72)
Allocation of resources: functional categories (% of total expenditure)					
Social Expenditures	0.78 (0.24)	1.35 (0.44)	0.89 (0.33)	0.29 (0.46)	2.29 (1.00)
of which:					
Health & sanitation	0.30 (0.17)	0.86 (0.33)	0.53 (0.24)	0.15 (0.35)	0.85 (0.53)
Education & culture	0.22 (0.17)	0.46 (0.31)	0.12 (0.20)	-0.04 (0.34)	0.98 (0.63)
Social welfare	0.16 (0.06)	0.27 (0.11)	0.28 (0.10)	0.13 (0.12)	0.36 (0.25)
Other expenditures:					
Housing	-0.12 (0.14)	-0.39 (0.33)	0.06 (0.25)	0.25 (0.36)	0.44 (0.75)
Transportation	-0.19 (0.11)	-0.02 (0.23)	-0.32 (0.15)	-0.15 (0.22)	-0.75 (0.33)
Other	-0.39 (0.27)	-1.07 (0.53)	-0.67 (0.39)	-0.37 (0.52)	-1.90 (1.24)
Social Expenditures per capita	0.77 (0.58)	2.83 (1.41)	0.94 (0.97)	-0.27 (1.54)	7.34 (2.85)
Observations (all)	8943	2395	4158	2081	919
Observations (effective)	4972	1319	2682	1353	460

Notes: Estimates from our baseline RD specification (equation 3.2), using the bias-corrected procedure of Calonico et al. [2014] and controlling for city and year fixed effects. Outcomes measured as 3-year averages over a mayoral term (excluding the first year of the term). Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Table C.9: RD estimates of the effect of a left-wing mayor - by city size

	Baseline	Subsamples: Population higher than ...			
		25th pct 6,594	median 14,196	75th pct 31,209	90th pct 76,530
Size of government: overall revenues and expenses					
Expenditure per capita	-0.01 (0.56)	-0.19 (0.62)	0.12 (0.77)	0.95 (1.18)	1.15 (1.88)
Expenditure, % of GDP	0.01 (0.24)	-0.16 (0.27)	-0.05 (0.16)	0.11 (0.19)	0.02 (0.21)
Revenue per capita	0.39 (0.52)	0.32 (0.60)	-0.12 (0.70)	0.21 (1.14)	0.18 (1.67)
Revenue, % of GDP	0.12 (0.25)	-0.03 (0.29)	-0.06 (0.15)	0.01 (0.19)	-0.02 (0.21)
Allocation of resources: budget categories (% of total expenditure)					
Current Expenditure	-0.05 (0.17)	0.13 (0.21)	0.04 (0.25)	-0.18 (0.34)	-0.32 (0.56)
of which:					
Personnel	-0.05 (0.22)	0.10 (0.23)	0.31 (0.29)	0.34 (0.44)	1.19 (0.74)
Public Investment	0.09 (0.16)	-0.04 (0.21)	0.14 (0.25)	0.34 (0.32)	0.17 (0.63)
Allocation of resources: functional categories (% of total expenditure)					
Social Expenditures	0.64 (0.21)	0.75 (0.25)	0.51 (0.29)	0.34 (0.40)	0.68 (0.91)
of which:					
Health & sanitation	0.18 (0.15)	0.17 (0.16)	0.05 (0.22)	0.17 (0.33)	-0.43 (0.62)
Education & culture	0.24 (0.16)	0.36 (0.19)	0.41 (0.22)	0.16 (0.27)	0.94 (0.51)
Social welfare	0.16 (0.06)	0.18 (0.06)	0.06 (0.06)	0.08 (0.09)	-0.03 (0.14)
Other expenditures:					
Housing	-0.16 (0.13)	-0.28 (0.16)	-0.30 (0.20)	-0.01 (0.28)	0.46 (0.54)
Transportation	-0.18 (0.09)	-0.25 (0.10)	-0.23 (0.12)	-0.35 (0.18)	-0.21 (0.25)
Other	-0.21 (0.23)	-0.15 (0.28)	0.05 (0.31)	-0.08 (0.47)	-0.88 (1.00)
Social Expenditures per capita	1.16 (0.61)	1.30 (0.71)	1.36 (0.86)	2.26 (1.33)	1.91 (2.10)
Observations (all)	8943	6707	4471	2235	894
Observations (effective)	4408	3714	2393	1137	420

Notes: Estimates from our baseline RD specification (equation 3.2), using the bias-corrected procedure of Calonico et al. [2014] and controlling for city and year fixed effects. Outcomes measured as 4-year averages over a mayoral term. Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. In the heading, numbers below the percentiles are the corresponding population thresholds. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Table C.10: RD estimates of the effect of a left-wing mayor: robustness to alternative bandwidth selection

Outcome	Baseline		Subsamples							
			Lame Duck		Tiebout < median		Ideology distance > median		Oil windfall	
	MSE	CER	MSE	CER	MSE	CER	MSE	CER	MSE	CER
Size of government: overall revenues and expenses										
Expenditure per capita	-0.01 (0.56)	0.05 (0.61)	0.52 (1.27)	0.99 (1.25)	-1.24 (0.85)	-1.07 (0.91)	0.23 (0.82)	-0.07 (0.91)	2.26 (2.06)	3.35 (2.22)
Expenditure, % of GDP	0.01 (0.24)	0.08 (0.21)	-1.05 (1.18)	-1.01 (1.21)	-0.36 (0.43)	-0.38 (0.47)	-0.02 (0.25)	-0.19 (0.39)	-0.13 (0.47)	0.04 (0.48)
Revenue per capita	0.39 (0.52)	0.62 (0.58)	0.59 (1.21)	1.10 (1.21)	-0.77 (0.77)	-0.68 (0.83)	1.02 (0.88)	1.03 (1.02)	2.19 (2.01)	3.12 (2.15)
Revenue, % of GDP	0.12 (0.25)	0.22 (0.23)	-1.14 (1.20)	-1.09 (1.23)	-0.20 (0.42)	-0.27 (0.48)	0.24 (0.27)	0.13 (0.42)	-0.21 (0.49)	0.09 (0.50)
Allocation of resources: budget categories (% of total expenditure)										
Current Expenditure	-0.05 (0.17)	-0.12 (0.18)	-0.39 (0.38)	-0.47 (0.41)	-0.15 (0.25)	-0.17 (0.27)	0.18 (0.31)	0.10 (0.34)	1.01 (0.65)	1.18 (0.68)
of which:										
Personnel	-0.05 (0.22)	-0.12 (0.24)	-0.81 (0.48)	-0.85 (0.52)	-0.04 (0.30)	-0.15 (0.33)	-0.08 (0.41)	-0.19 (0.45)	-0.34 (0.85)	-0.46 (0.91)
Public Investment	0.09 (0.16)	0.17 (0.18)	0.40 (0.37)	0.45 (0.40)	0.23 (0.26)	0.29 (0.29)	-0.08 (0.31)	-0.05 (0.34)	-0.96 (0.63)	-1.11 (0.66)
Allocation of resources: functional categories (% of total expenditure)										
Social Expenditures	0.64 (0.21)	0.75 (0.24)	1.27 (0.40)	1.39 (0.44)	0.71 (0.30)	0.71 (0.33)	0.91 (0.39)	1.00 (0.43)	2.19 (0.87)	2.21 (0.92)
of which:										
Health & sanitation	0.18 (0.15)	0.15 (0.17)	0.69 (0.30)	0.83 (0.32)	0.38 (0.22)	0.22 (0.25)	-0.11 (0.27)	-0.14 (0.30)	0.45 (0.42)	0.75 (0.47)
Education & culture	0.24 (0.16)	0.37 (0.18)	0.31 (0.30)	0.32 (0.32)	0.12 (0.19)	0.11 (0.21)	1.00 (0.30)	0.97 (0.35)	0.83 (0.50)	0.88 (0.54)
Social welfare	0.16 (0.06)	0.11 (0.07)	0.32 (0.11)	0.30 (0.12)	0.27 (0.09)	0.27 (0.10)	0.12 (0.10)	0.09 (0.11)	0.36 (0.20)	0.35 (0.21)
Other Expenditures:										
Housing	-0.16 (0.13)	-0.19 (0.14)	-0.36 (0.29)	-0.40 (0.32)	0.05 (0.21)	0.12 (0.23)	-0.15 (0.24)	-0.07 (0.26)	0.41 (0.65)	0.43 (0.70)
Transportation	-0.18 (0.09)	-0.18 (0.10)	-0.07 (0.20)	-0.01 (0.21)	-0.24 (0.13)	-0.21 (0.15)	-0.52 (0.19)	-0.58 (0.21)	-0.73 (0.29)	-0.75 (0.31)
Other	1.16 (0.61)	1.51 (0.67)	3.34 (1.36)	4.19 (1.39)	0.25 (0.86)	0.43 (0.93)	1.92 (0.93)	1.83 (1.04)	6.48 (2.40)	7.10 (2.64)
Social Expenditures per capita	1.16 (0.61)	1.51 (0.67)	3.34 (1.36)	4.19 (1.39)	0.25 (0.86)	0.43 (0.93)	1.92 (0.93)	1.83 (1.04)	6.48 (2.40)	7.10 (2.64)
Observations (all)	8943	8943	2395	2395	4158	4158	3105	3105	919	919
Observations (effective)	4408	3136	1227	929	2367	1810	1660	1209	451	360

Notes: Estimation of equation 3.2, using the Calonico et al. [2014] procedure and controlling for city and year fixed effects. Outcomes are 4-year averages over a mayoral term. Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

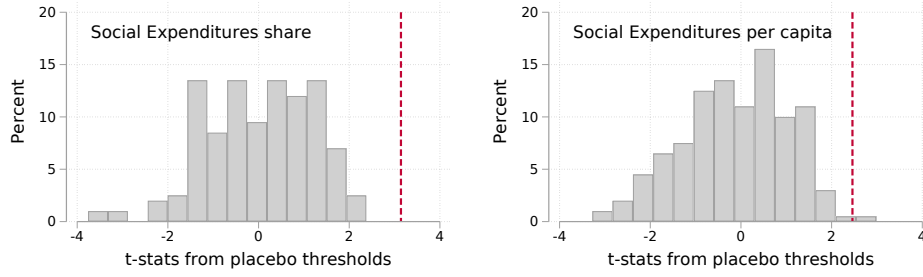
Table C.11: RD estimates of the effect of a left-wing mayor in the Tiebout and ideology distance subsample

	Baseline	Tiebout competition		Ideology distance	
		< median	< 25th pct	> median	> 75th pct
Size of city government					
Expenditure per capita	-0.01 (0.56)	-1.24 (0.85)	-1.21 (1.22)	0.23 (0.82)	-0.90 (1.13)
Expenditure, % of GDP	0.01 (0.24)	-0.36 (0.43)	-1.10 (0.89)	-0.02 (0.25)	0.43 (0.40)
Revenue per capita	0.39 (0.52)	-0.77 (0.77)	-0.44 (1.20)	1.02 (0.88)	0.70 (1.73)
Revenue, % of GDP	0.12 (0.25)	-0.20 (0.42)	-0.94 (0.88)	0.24 (0.27)	0.64 (0.58)
Allocation of resources: budget categories (% of total expenditure)					
Current Expenditure	-0.05 (0.17)	-0.15 (0.25)	0.40 (0.39)	0.18 (0.31)	0.16 (0.45)
of which:					
Personnel	-0.05 (0.22)	-0.04 (0.30)	0.32 (0.43)	-0.08 (0.41)	0.13 (0.63)
Public Investment	0.09 (0.16)	0.23 (0.26)	-0.36 (0.43)	-0.08 (0.31)	0.05 (0.43)
Allocation of resources: functional categories (% of total expenditure)					
Social Expenditures	0.64 (0.21)	0.71 (0.30)	0.20 (0.41)	0.91 (0.39)	0.78 (0.49)
of which:					
Health & sanitation	0.18 (0.15)	0.38 (0.22)	-0.02 (0.31)	-0.11 (0.27)	-0.36 (0.38)
Education & culture	0.24 (0.16)	0.12 (0.19)	-0.00 (0.31)	1.00 (0.30)	1.22 (0.43)
Social welfare	0.16 (0.06)	0.27 (0.09)	0.15 (0.11)	0.12 (0.10)	0.14 (0.14)
Other Expenditures:					
Housing	-0.16 (0.13)	0.05 (0.21)	0.07 (0.31)	-0.15 (0.24)	0.06 (0.37)
Transportation	-0.18 (0.09)	-0.24 (0.13)	-0.15 (0.18)	-0.52 (0.19)	-0.13 (0.24)
Other	-0.21 (0.23)	-0.59 (0.34)	-0.08 (0.47)	-0.22 (0.40)	-0.59 (0.53)
Social Exp. per capita	1.16 (0.61)	0.25 (0.86)	-0.74 (1.37)	1.92 (0.93)	0.77 (1.36)
Observations (all)	8943	4158	2081	3105	1545
Observations (effective)	4408	2367	1347	1660	814

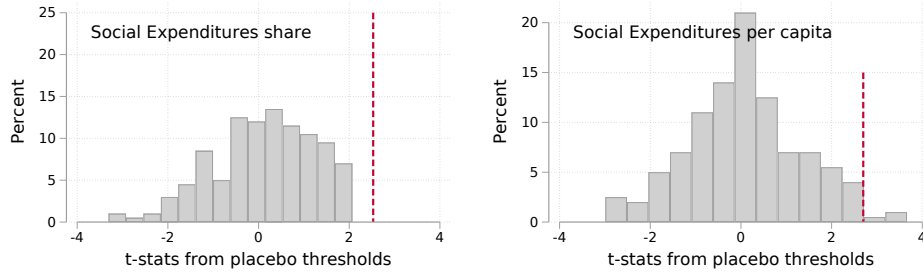
Notes: Estimation of equation 3.2, using the Calonico et al. [2014] procedure and controlling for city and year fixed effects. Outcomes are 4-year averages over a mayoral term. Per-capita variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

Figure C.6: Falsification test using placebo thresholds - effect on social expenditures, subsamples

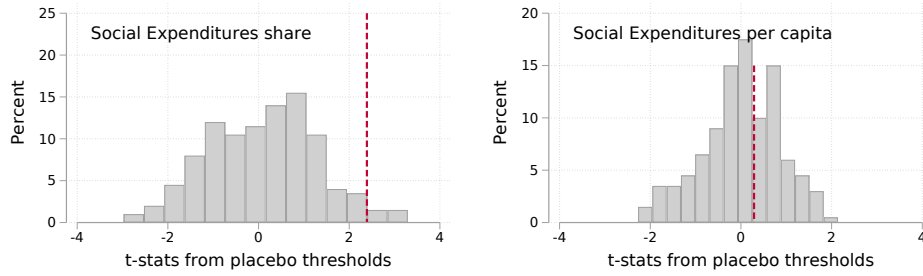
(a) Cities with a lame-duck mayor



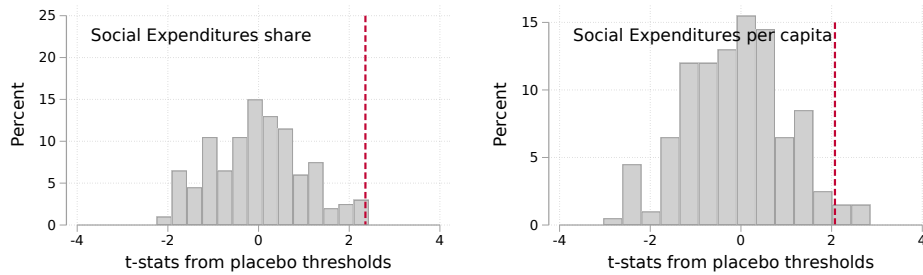
(b) Cities experiencing oil windfalls



(c) Cities facing below-median Tiebout competition



(d) Cities with above-median coalition ideology distance



Notes: Empirical distribution of t-statistics from our RD estimates (equation 3.2) of the effect of a left-wing mayor on the share of social spending and social expenditure per capita, based on 200 randomly-drawn placebo thresholds, drawn separately on the left and on the right side of the true threshold (100 on each side), using only observations belonging to that side and with at least 25 observations on each side of the bandwidth. Vertical line = t-statistics obtained using the true threshold. The t-statistics are from the robust bias-corrected procedure of Calonico et al. [2014].

RD estimates on welfare-related outcomes

To provide a broader picture of how partisanship affects policy in Brazilian cities, in this appendix we look at welfare-related outcomes provided by municipalities. First, we discuss the data sources and define the variables we use. Then, Appendix Table C.12 reports summary statistics and Appendix Table C.13 presents the regression discontinuity results.

Educational Outcomes

To assess the impact of partisanship on welfare-related outcomes in the area of education, we create two types of indicators using data from the *Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira* (INEP).

The first type of indicators is related to the infrastructure and human resources provided by city governments. We proxy infrastructure by the number of child care facilities, preschools and primary schools per a hundred thousand residents. For primary schools, we also compute the average classroom size, which is defined as the total number of students enrolled in 1st to 5th grades in municipal schools by the total number of classrooms available in the municipal school system for these grade levels. The (enrolled) students-to-teachers ratio is our measure of human resources available in the municipality. All these indicators are obtained from the School Census, an annual survey of all schools in Brazil, and only use data from municipal schools.

The second type of indicators attempts to capture the overall performance of the municipal educational system. In this case, we use data from the *Índice de Desenvolvimento da Educação Básica* (IDEB), which is an index created in 2005 to monitor student achievement and progression flows at primary and lower secondary education. IDEB assigns a score between 0 and 10 to individual schools and school systems. The index consists of two subindices: test scores and grade progression. The IDEB test scores are based on standardized average test scores on math and Portuguese from SAEB and Prova Brasil, which are national standardized exams administered every two years since 2005. These exams are taken by students in the last year of primary school, middle school and high school from schools with at least 20 students enrolled in that particular grade-level. The IDEB progression rate is defined as the inverse of the average time to complete a grade level and is obtained from the School Census [Fernandes, 2007].

IDEB is only available for the first and third year of a mayoral term. Given that it would be really hard for mayors to affect any of these outcomes in the first year after the

election, we use all indicators measured at the mayors 3rd year in office. As we pointed out in Section 3.2, cities are responsible for child care, primary and middle school education in Brazil. We focus on primary schools (1st to 5th grades), however, as many cities in our sample do not have middle schools in the municipal school system.

Health Outcomes

Similarly to education, we use two type of indicators to assess the impact of partisanship on welfare-related health outcomes. Also in this case, we use measures related to infrastructure and human resources and an overall measure of performance of the health care system in the city.

We proxy infrastructure in the municipal health care system by the number of clinics per a hundred thousand residents. We compute this outcome for clinics providing basic services (low complexity) and the total number of clinics. We do not use hospitals for two reasons. First, as discussed in Section 3.2, cities are responsible for the provision of basic health services. Second, most cities in our sample do not have a municipal hospital. We use two measures of human resources. The number of teams of the Family Health Program (ESF) in the city and the total number of doctors working for the municipal health-care system. ESF targets prevention and provision of basic health through the use of professional health-care teams directly intervening at the community level. Each team is assigned a predetermined number of families and focus on the provision of counseling, prevention, orientation related to recovery, and advice for fighting frequent diseases and for overall health protection in the community. It is important to note that ESF is run by the federal government and implementation requires voluntary adhesion of the municipal administration [Rocha and Soares, 2010]. Both measures are scaled by a hundred thousand residents. To capture the overall performance of the municipal health-care system, we use the infant mortality rate defined as the number of infant deaths for every thousand live births.

All welfare-related outcomes of the health care system are obtained from DATASUS, a database from the Ministry of Health. All outcomes are computed as the mayoral term average, except for ESF which is measured at the last year in office due to data availability.

Law Enforcement Outcomes

Almost fifty eight thousand homicides were registered in Brazil in 2018 (the last year for which this statistic is available), which corresponds to an homicide rate of 27.8 homicides

per hundred thousand people. The homicide rate was even higher in the previous four years [Cerqueira et al., 2020]. We thus assess the overall performance in the law enforcement area using the homicide rate at the city level obtained from *Atlas da Violência* from the Institute for Applied Economic Research (IPEA).

Table C.12: Welfare-related outcomes – descriptive statistics

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Education outcomes					
Student-teachers ratio	22.75 (13.05)	22.60 (9.87)	23.23 (9.85)	22.91 (10.83)	26.07 (13.51)
Average classroom size	23.40 (12.19)	23.27 (8.47)	23.84 (8.38)	23.52 (9.86)	26.01 (12.70)
Teachers, per 100K res.	352.98 (158.59)	351.00 (163.78)	342.17 (152.74)	341.68 (156.78)	353.47 (139.77)
Schools, per 100K res.	92.40 (90.70)	92.74 (92.23)	79.93 (87.54)	89.05 (86.78)	105.78 (86.72)
Child Care, per 100K res.	25.50 (34.51)	26.60 (35.97)	23.41 (30.88)	24.25 (32.59)	27.66 (35.51)
Prepresch, per 100K res.	71.83 (71.52)	72.67 (70.67)	60.98 (64.16)	68.94 (66.99)	82.13 (71.12)
IDEB test scores	5.18 (0.88)	5.18 (0.87)	5.28 (0.88)	5.17 (0.87)	4.92 (0.83)
IDEB progression rate	0.89 (0.09)	0.89 (0.08)	0.90 (0.08)	0.89 (0.08)	0.84 (0.10)
IDEB index	4.65 (1.10)	4.66 (1.08)	4.77 (1.09)	4.65 (1.09)	4.20 (1.08)
Health outcomes					
Clinics (basic), per 100K res.	74.14 (44.72)	73.60 (45.43)	72.35 (46.36)	73.74 (46.52)	65.64 (38.39)
Clinics (total), per 100K res.	118.07 (81.71)	118.02 (79.82)	121.79 (85.11)	120.57 (85.71)	98.95 (64.33)
ESF team, per 100K res.	7.18 (8.90)	6.64 (8.72)	7.33 (8.92)	7.02 (8.52)	7.21 (8.37)
Doctors, per 100K res.	44.90 (53.31)	46.07 (53.80)	54.94 (63.60)	47.06 (55.34)	53.78 (66.94)
Infant mortality rate	15.38 (6.94)	15.21 (6.76)	15.05 (6.47)	15.16 (6.52)	15.90 (6.15)
Law enforcement outcomes					
Homicide rate	20.18 (16.41)	20.05 (16.27)	22.03 (17.27)	21.07 (16.75)	24.53 (18.71)

Notes: This table reports mean and standard deviation (in parenthesis) for welfare-related outcomes. Data on education outcomes are from INEP. Data on health outcomes are from DATASUS. Homicides rate are from IPEA. The number of observations available for each welfare-related outcome is presented in Appendix Table C.13. See Section 3.6 for the specific definition and motivation of each subsample.

Table C.13: RD estimates of the effect of a left-wing mayor on welfare-related outcomes

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Education outcomes					
Student-teachers ratio	-1.51 (1.05)	0.35 (2.13)	-1.08 (1.49)	-1.67 (1.70)	-2.72 (3.34)
Average classroom size	-2.26 (1.31)	-2.91 (2.21)	-2.93 (1.71)	-3.95 (2.06)	-3.75 (3.61)
Teachers, per 100K res.	0.32 (1.16)	-2.21 (2.24)	0.70 (1.67)	2.29 (2.24)	3.46 (3.81)
Schools, per 100K res.	-3.43 (1.27)	1.90 (2.70)	-3.01 (1.76)	-4.41 (2.15)	0.90 (3.68)
Child Care, per 100K res.	-1.49 (2.66)	7.80 (5.40)	0.87 (3.52)	-3.71 (4.26)	-6.80 (8.14)
Pre Schools, per 100K res.	-1.45 (1.87)	-0.74 (3.79)	-6.70 (3.08)	0.07 (3.12)	-4.13 (5.78)
Observations (all)	8872	2370	4125	3086	918
Observations (effective)	5298	1280	2306	1620	489
IDEB progression rate	0.33 (0.38)	0.39 (0.70)	0.78 (0.50)	0.61 (0.51)	2.01 (1.20)
Observations (all)	8292	2221	3926	2905	901
Observations (effective)	3841	1070	1710	1894	390
IDEB test scores	-0.35 (0.40)	-0.01 (0.83)	-0.49 (0.67)	0.34 (0.71)	1.70 (1.16)
IDEB overall index	-0.02 (0.57)	0.30 (1.02)	0.01 (0.71)	1.21 (0.92)	3.18 (1.69)
Observations (all)	8211	2196	3894	2875	900
Observations (effective)	4385	1206	2531	1632	444

Table C.13: RD estimates of the effect of a left-wing mayor on welfare-related outcomes

	Baseline	Subsamples			
		Lame Duck	Tiebout < median	Ideology distance > median	Oil windfall
Health outcomes					
Clinics (basic), per 100K res.	0.80 (1.54)	-1.21 (2.88)	3.73 (2.15)	-0.20 (2.20)	-2.23 (3.13)
Clinics (total), per 100K res.	0.34 (1.60)	-2.54 (3.35)	2.15 (2.23)	-0.05 (2.56)	-4.64 (4.04)
Observations (all)	8937	2395	4155	3104	919
Observations (effective)	3998	985	1874	1765	455
ESF team, per 100K res.	-2.62 (2.88)	0.36 (6.52)	-0.70 (4.52)	-6.50 (5.58)	-2.79 (8.96)
Observations (all)	4120	1069	2151	1522	517
Observations (effective)	2579	532	1275	843	282
Doctors, per 100K res.	-0.60 (3.37)	3.67 (6.81)	-13.39 (5.57)	4.06 (5.55)	-9.61 (10.27)
Observations (all)	8033	2132	3860	2850	860
Observations (effective)	4379	1132	1617	1726	357
Infant mortality	-2.75 (2.26)	-1.95 (3.86)	0.60 (2.92)	2.87 (3.42)	-6.14 (5.68)
Observations (all)	8656	2313	4065	3012	914
Observations (effective)	4525	1357	2114	1565	472
Law enforcement outcomes					
Homicide rate	-4.76 (2.93)	2.06 (5.53)	5.56 (3.90)	5.27 (3.95)	-6.00 (8.18)
Observations (all)	8187	2181	3895	2884	895
Observations (effective)	3849	1003	2004	1764	471

Notes: Estimates from our baseline RD specification (equation 3.2), using the bias-corrected procedure of Calonico et al. [2014] and controlling for city and year fixed effects. Education outcomes measured in the 3rd year in office. Homicide rates and health outcomes, except for number of ESF teams, measured as 4-year averages over a mayoral term. ESF teams measured in the 4th year in office. All welfare-related outcome variables are taken in logs and multiplied by 100, so coefficients represent percentage-points differences. Robust and bias-corrected standard errors clustered by municipality in parenthesis.

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